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CONTENTS

R. L. REID. A Test of Sensory Pre-Conditioning in Pigeons - - - - - - - - 49

E. C. POULTON and R. L. GREGORY. Blinking during Visual Tracking - - - - - 57

T. E. CLEGHORN and H. D. DARCUS. The Sensibility to Passive Movement of the Human Elbow Joint - - - - - - - 66

F. KNÖPFELMACHER. Some Effects of Reward on the Strength of Position Stereotypes in the White Rat - - - - - - 78

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CONTENTS

I. M. L. HUNTER. Discrimination Learning and Transposition Behaviour of Rats in a Water Tank Apparatus - - - - - - - - 91

D. SHEPPARD and G. W. SCOTT BLAIR. Subjective Assessments of Firmness - - - - 101

K. R. L. HALL, A. E. EARLE and T. G. CROOKES. A Pendulum Phenomenon in the Visual Perception of Apparent Movement - - - - - - 109

L. BURKE. On the Tunnel Effect - - - 121

K. H. SPRING. An Apparatus for Measuring the Steadiness of Aim of a Rifle - - - - - 139

Book Review - - - - - - - - 141

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Vol. IV Part 4 November 1952

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CONTENTS

MARIANO YELA. Phenomenal Causation at a Distance 139
R. B. AMMONS. Relationship of Motivation and Method of Testing to Distribution of Practice Phenomena in Rotary Pursuit - - - - - 155
R. L. GREGORY. Variations in Blink Rate During Non-Visual Tasks - - - - - - - 165
W. T. POLLOCK and A. CHAPANIS. The Apparent Length of a Line as a Function of its Inclination - - 170
K. H. SPRING. An Apparatus for Measuring the Steadiness of Aim of a Rifle - - - - - 179
Book Reviews - - - - - - - - - 181

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THE QUARTERLY JOURNAL OF
EXPERIMENTAL PSYCHOLOGY

VOL. IV 1952 PART 2

A TEST OF SENSORY PRE-CONDITIONING IN PIGEONS

BY

R. L. REID

(*University of Edinburgh*)

478 L

Sixteen pigeons were used in an experiment designed to show the sensory pre-conditioning effect as originally reported by Brogden. An experimental group received 200 simultaneous presentations of a buzzer paired with a light stimulus. They were then trained to respond to one of these stimuli alone and tested for response to the other. A control group received the same treatment except in the initial stage when 200 presentations of the test stimulus only were given. The results provided no evidence that the pairing of stimuli affected behaviour during the critical test.

Both groups responded to the test stimulus to a similar degree in this experiment, whereas in Brogden's original study with dogs experimental animals responded significantly more frequently than did their controls. The discrepancy in results can be attributed to the use of different control procedures in the two experiments. Brogden's controls were not exposed to presentations of the test stimulus before training. Differences in familiarity with this stimulus may have produced the differences between the behaviour of his control and experimental animals. This interpretation is supported by the result of a preliminary experiment with pigeons in which Brogden's control procedure was used and his original results confirmed.

With human subjects there is some evidence that pairing procedures may result in enhanced generalization, alterations in sensory thresholds or hallucinations. However, these effects are little understood and difficult to predict. Although no functional relationships like those found in ordinary conditioning have yet been shown to apply, the terms "sensory conditioning" and "sensory pre-conditioning" have been widely used and the data have been quoted in support of theories of learning that require the setting up of direct sensory-sensory relationships.

Until there is unequivocal evidence of pairing effects with animals or fuller knowledge of the occurrence of such effects with human subjects, it is considered unadvisable to link them even by name with basic learning mechanisms such as conditioning.

INTRODUCTION

THE term *sensory pre-conditioning* was first used by Brogden (1939) reporting an experiment with dogs. Eight animals forming an experimental group received 200 combined presentations of a bell and a light. Using an avoidance conditioning procedure with shock as the unconditioned stimulus, a reliable response was then set up to one of these stimuli presented alone. In a critical test the other stimulus was presented and a measure taken of the number of responses made to it in extinction. Eight control animals did not receive the initial paired stimulations, but were treated similarly otherwise. The results showed that experimental animals responded to the test stimulus to a significantly higher degree than controls.

Brogden pointed out that conditions like those of the experiment would often be found in the everyday life of any organism. For this reason he considered his findings important. The significance of the phenomenon for learning theories in general was not discussed.

Human subjects have been used in similar experiments by Brogden (1942), (1947), Karn (1947), Chernikoff and Brogden (1949), and Brogden (1950). Karn's procedure most closely followed that of the original work with dogs. An experimental group of subjects received 50 presentations of a light and a buzzer stimulus in combination. Finger withdrawal to the buzzer was then conditioned using electroshock. Finally, subjects were tested for response to the light on 10 trials given without shock, although the electrodes remained in position. A control group missed out the first stage of the experiment. During the test, experimental subjects averaged over six responses to the light, controls less than one response.

Brogden (1942) used the galvanic skin response in a first attempt with human subjects and failed to show any effect of pairing stimuli. His second experiment (1947) was successful. Subjects were told that their reaction times were to be measured and that they would receive a weak shock for slow responses. On 30 trials they responded to the lighting up of a milk-glass plate by pressing a telegraph key. No shocks were given. Before this part of the experiment, while the experimenter was supposedly adjusting the apparatus, subjects of the experimental group saw the light go on and at the same time heard a low frequency tone on 10 occasions. Subjects of one control group heard the tone only, under similar circumstances. Other control subjects started without any preliminaries. In all cases, after the 30 reactions to the light stimulus, the tone was presented 10 times. The mean frequency of response of the experimental group was reliably greater than that of other groups at better than the 5 per cent. confidence level.

Chernikoff and Brogden (1949) followed the same procedure with greater success, obtaining results of equal statistical significance with 10 subjects per group instead of 42 as in the previous experiment. They attributed this increase in efficiency to the use of an auditory stimulus that could not be localized. (The rationale is not clear.) Their experiment was also designed to relate the magnitude of the effect produced by pairing stimuli to three different sets of instructions given to subjects. Two experimental groups were told that the stimulus would change to a tone during the course of the experiment. One was instructed not to respond, and the other to do what seemed natural. Neither group gave a frequency of responses differing significantly from that of the appropriate control group. Increased responding that could be attributed to paired presentations of stimuli occurred only among those experimental subjects who did not know that the tone would occur.

Finally, Brogden (1950) reported two further experiments designed to investigate the effect of varying the number of pre-training stimulus combinations and the number of trials used in training. The results of both were inconclusive and led to a third experiment on familiar lines, but with the addition of a new test procedure. On 10 trials, subjects of the experimental group listened to a faint tone whose occurrence was made to coincide with a slight increase in the illumination level of the experimental room. A control group experienced only the tone. All subjects then received 30 presentations of the tone and pressed a telegraph key to indicate each time it was heard. Ten presentations of the light stimulus and 10 of the tone in random order were given as a test. Control and experimental groups did not differ in the number of key-pressings made to the light, i.e. sensory pre-conditioning was not revealed by the usual measure of response frequency. However, when the experiment was extended to include determinations of auditory acuity under two conditions, subjects of the experimental group showed a significant increase in acuity when the light stimulus was present over acuity measured in its absence, while subjects of the control group showed no comparable difference in acuity. Some relationship between tone and light appeared to have been set up as the result of their joint presentation during the initial stage of the experiment. For Brogden, it remained an open question whether an alteration of thresholds as observed in this experiment could be regarded as indicating the same sort of relationship between stimuli that was inferred in the earlier reports from measurements of frequency of response.

This last study is rather like the traditional experiments on "Suggestion" and is closely related to work by Ellson (1941), who studied the effect of pairing stimuli in producing hallucinations. A light and a faint tone presented together on 60 trials to an experimental group of subjects resulted in their reporting the tone when the light alone was given, with greater frequency than occurred in control groups not exposed to the initial paired trials. (With Brogden's subjects thresholds were reduced; with Ellson's eliminated.) It is clear, however, from the reports of subjects showing pairing effects as measured by the frequency of key-pressing (Brogden, 1947) that responses to the test stimulus *as if* it were the training stimulus are not necessarily mediated by an hallucination of the latter. (With animals as subjects, there are no clear criteria for differentiating an "as if" from an hallucinatory response.)

The experiments summarized above have received a fair amount of attention in the literature on learning. Those who carried out the experiments have been consistently cautious about assessing their theoretical significance. However, others have used the data to support theories of learning that involve an essentially afferent type of modification through contiguity of stimuli. Birch and Bitterman (1949, 1951) have been particularly enthusiastic. "The results of the sensory pre-conditioning experiments require us to postulate a process of afferent modification (sensory integration) . . . which takes place independently of need reduction" (1949, p. 302). "From our point of view the latent learning experiment may be understood as a complication of the sensory pre-conditioning experiment . . . not only a contiguity principle, but an afferent contiguity principle, is required by this evidence" (1951, p. 360). Certainly these seem to be correct inferences. But before making them it would be advisable to consider whether the pre-conditioning effect can be regarded with any certainty as exemplifying a basic learning process.

The experiment reported here was originally undertaken for this purpose. First it seemed necessary to show that the effect does occur with an organism simple enough to make an explanation in terms of symbolic functioning unlikely. Second, for the effect to be related to ordinary conditioning the extent to which it occurred would have to be related to the number of paired stimulations given in the pre-training period. This was regarded as a long range objective.

Preliminary work with eight pigeons was carried out by Macpherson (1950). Experimental animals received 200 combined stimuli (light and buzzer). They were then trained, using food rewards, to peck in the presence of one of these stimuli and tested for response to the other. A control group was given similar treatment, but without the pre-training combinations of stimuli. The results confirmed those of Brogden (1939). Measured from a baseline of slow random pecking when neither stimulus was present, all birds of the experimental group increased their rate of pecking when the test stimulus was presented, while two controls gave a very slight increase and two a larger decrease. Taking the groups as a whole, the increased rate of pecking of the experimental group was of the order expected by chance only in one or two cases in 100—while a slight overall decrease in rate of pecking found with the control group was to be expected in four cases out of 10. Despite the small numbers used, this confirmed Brogden's results for a different organism under rather different conditions.

In an experiment reported in detail below, more elaborate apparatus was used, and the control-group treatment was altered so as to ensure that both groups of animals should have equal familiarity with the stimulus used in testing. The results were negative.

EXPERIMENT

Subjects and Apparatus

Sixteen male homing pigeons about one year old were used. Throughout the experiment, all birds were kept on a schedule of 12 hours darkness to 12 hours light.

The apparatus was a modified type of Skinner Box, 18 in. × 15 in. × 30 in., sound-deadened by a layer of glass-wool between double walls. A removable top was sealed with sponge rubber under compression to the body of the box. A rotary pump outside the box provided a steady flow of air and a masking noise of low intensity.

Thin aluminium walls divided the interior of the box into three compartments. The first contained the auditory stimulus—a 4 v. buzzer. The second, a space 12 in. × 12 in. × 15 in. high with matt-grey walls, formed the experimental cage proper. This cage was illuminated indirectly by a 30 w. "Striplight" concealed in the roof, and directly by a small 15 w. bulb on the rear wall shining through milk perspex. On the front wall, 8 in. from the floor, a square of 1 in. side had been removed and the hole covered by a hinged perspex window through which a small blue triangle on a white ground was displayed. This acted as a target for pecking. Relay keys fitted to the window closed whenever slight pressure was exerted on it from the bird's side of the wall. The third compartment contained a 24 v. relay operating an arm that released maple peas from a magazine into a food pan on the bird's side of the dividing wall. Pecking at the window could be made to operate the relay through a "flip-flop" circuit whose action was to preserve the brief contact made by the bird long enough for the relay to operate. All pecks were automatically recorded by an electrical counter or a slow-speed ink-writer.

Stimuli consisted of sounding the buzzer for 1½ seconds (B), or reducing the general illumination of the cage by switching off the concealed roof light for the same period (L), or both in combination (B + L). An objection may be made to this and other similar procedures—that (B + L) may form a stimulus pattern rather than two discrete stimuli occurring at the same time. But an attempt with four birds to obtain responses to the combined stimuli and not to either alone met with only limited success after many trials, while on the other hand birds trained to either L or B alone invariably responded to the combination.

The presentations were made automatically by a modified Venner switch located outside the experimental room. This switch was also used to set the reinforcing circuit so that, during training, pecking within 10 seconds of the presentation of stimuli would cause delivery of food. The switching gear rotated once every six minutes giving four contacts at irregular intervals.

Procedure

Subjects were divided into two control and two experimental groups designated BC, LC, BE and LE, according to the stimulus to which the pecking response was established during training. Table I shows stimuli used with each group at different stages of the experiment.

TABLE I

STIMULUS CONDITIONS

	BC	*LC*	*BE*	*LE*
Pre-training ..	L	B	(B + L)	(B + L)
Training	B	L	B	L
Test	L	B	L	B

1. *Pre-training.*—During this phase of the procedure each bird was maintained by limited feeding at 90 per cent. of its average weight as measured during seven days of unlimited access to food. (A 10 per cent. reduction is sufficient to eliminate the comatose state of a replete bird, but does not represent a degree of hunger sufficient for training instrumental responses). After a preliminary habituation session, birds were placed in the experimental box on 10 consecutive days and given 20 presentations of stimuli as detailed

in Table I, (B + L) for all experimental group birds and B or L alone for controls. All stimuli lasted 1½ seconds. Presentations took place at irregular intervals, averaging 1½ minutes, and started 10 minutes after the bird entered the box. Only in this part of the experiment did control and experimental groups receive different treatment. Between pre-training and training, a further 10 per cent. reduction in body weight was made. This usually required three days.

2. *Training.*—Since pecking at the key was not within the behavioural repertoire of the animals used, a response of picking up grain from the food-pan was first obtained and from the increased general activity that resulted a pecking response was built up by reinforcing successively nearer and nearer approximations to it. By the end of a session lasting from 25 to 35 minutes all birds were pecking and operating the reinforcement mechanism. On the next day discrimination training was started using the stimulus appropriate for each group as shewn in Table I. Only the first peck occurring within 10 seconds of the termination of the stimulus was reinforced. After three to six daily discrimination periods lasting two hours each, plus two extinction periods of four hours when no stimuli were presented, all birds reached the training criterion, viz.: on 25 consecutive trials pecking at least 24 times within five seconds of the termination of the stimulus, and pecking not more than once during the equivalent 25 periods of five seconds duration preceding its onset.

3. *Test.*—Immediately after the training criterion had been reached, a change-over was made to the test stimulus which was presented 25 times under the same conditions as in training, except that no reponses were reinforced.

TABLE II

TRAINING DATA AND TEST RESULTS

SUBJECTS	TRAINING		TEST RESULTS		
	Sessions Required	*Food given (gms.)*	(a) *Responses preceding stimuli*	(b) *Responses following stimuli*	*Score* (b − a)
BC 1 ..	4	50	1	3	2
,, 2 ..	4	39	0	4	4
,, 3 ..	5	52	0	0	0
,, 4 ..	3	45	2	1	− 1
Total ..	16	186	3	8	5
LC 1 ..	5	38	2	8	6
,, 2 ..	6	53	1	4	3
,, 3 ..	4	45	1	0	− 1
,, 4 ..	3	42	0	2	2
Total ..	18	178	4	14	10
BE 1 ..	4	52	2	0	− 2
,, 2 ..	4	38	0	0	0
,, 3 ..	3	35	0	5	5
,, 4 ..	5	58	0	1	1
Total ..	16	183	2	6	4
LE 1 ..	5	65	1	4	3
,, 2 ..	4	50	1	4	3
,, 3 ..	5	55	0	3	3
,, 4 ..	3	40	0	1	1
Total ..	17	210	2	12	10

Results

Training data and test results for the 16 birds are given in Table II. Figures in the first two columns show that there were no serious differences between groups either in the number of training sessions required or in the weight of food given in reinforcement before the training criterion was reached. This is of some importance since generalization is known to vary with amount of training.

The last three columns of Table II provide data on responses made during the test period. Figures in column (*a*) indicate the number of times pecking occurred during 25 periods of five seconds' duration preceding presentations of the test stimulus, while column (*b*) gives the same information for the 25 corresponding periods following its termination. In both cases, the maximum for each bird would be 25, since the occurrence of pecking during a period was counted as *one* regardless of whether the bird tapped the window only once or produced a short burst of pecking. By subtracting figures in column (*b*) from those in column (*a*) a score was obtained, indicating for each bird the effect of presenting the test stimulus as measured from a baseline set by its own frequency of random pecking. The three negative scores are cases where presenting the stimulus tended to make pecking less rather than more likely to occur.

The results provide no evidence for any effect of the contiguous presentations of stimuli given to experimental animals. The two experimental groups made scores of 4 and 10, while the corresponding control groups scored 5 and 10. The total scores, 14 for eight experimental birds and 15 for eight controls, indicate only the occurrence of a small degree of sensory generalization between stimuli in different modalities.

It is interesting to note that generalization was asymmetrical and that Brogden (1939) found a similar lack of symmetry. The eight birds trained to respond to the light generalized to the buzzer more (score 20) than birds trained to respond to the buzzer generalized to the light (score 9).

Note

Negative results tend to raise doubts about the sensitivity of the measures used. With the Skinner-Box type of apparatus, rate of responding is a more normal index of behaviour than that used in Table II. However, rate, i.e. number of pecks occurring per unit of time, was in fact recorded continuously during the experiment and was found to correlate highly with the simpler measure of whether the bird pecked or did not peck during a restricted period. The index used was adopted, since it gave the same information as rate and had the advantage of being more meaningful by fixing scores within definite limits. Hefferline (1950) has used a similar index in dealing with lever-pressing responses of rats and discusses the use of the burst of responses rather than the single response as a behavioural unit.

In case some tendency for the experimental birds to respond to the test stimulus more than controls might exist, but be undetected by the method used, a further experiment was performed. Seven of the control and six of the experimental birds were presented 15 times with the test stimulus. Simultaneously food was delivered. This procedure resulted in greatly increasing the probability of pecking on presenting the stimulus without food. On five periods, each of 5 seconds' duration measured from presentation of the stimulus, ex-control birds averaged 2·9 responses and ex-experimental 2·3. The response had been strengthened to a similar degree in both groups.

DISCUSSION

Experiments with Animals

No confirmation was obtained of Brogden's earlier finding with dogs. The experiment reported here differs in many respects from Brogden's. These differences are perhaps too obvious to require discussion. However, if sensory pre-conditioning is to be regarded as a basic learning process there is no reason to suppose any of them should be crucial. Further, Macpherson's experiment with pigeons provided a positive result like that of Brogden.

If we ask why one of the pigeon experiments succeeded while the other failed, the most obvious answer is that the control-group techniques were different. In Macpherson's experiment, control and experimental subjects were not equally familiar with the test stimulus, while in the main experiment reported a control bird tested with buzzer or light had as much previous experience of it as had the corresponding experimental bird. Macpherson's results can be interpreted as showing that birds generalize more to a familiar than to an unfamiliar stimulus. Since Macpherson's and Brogden's control group techniques were similar, the same interpretation can be put on Brogden's results.

The negative result obtained with the improved control procedure is not to be regarded as in any way establishing that pre-conditioning does not occur, yet it shows clearly the unsatisfactory nature of Brogden's original experimental design. Pre-training exposures to the test stimulus rather than to its combination with the training stimulus may have been the variable that led to his experimental animals responding more frequently than their controls.

With animals, a good case may be made for suspending judgment on the effectiveness of pairing stimuli until a successful experiment is performed, using a control procedure that rules out the influence on generalization of unfamiliarity with the test stimulus.

Experiments with human subjects

With human subjects, Brogden has obtained positive results using the improved control procedure, so that the ambiguity of the original animal experiment is avoided. However, other criticisms become relevant. In Brogden's 1950 experiment, the pairing effect was measured as a lowering of an auditory threshold to a tone in the presence of a light stimulus. The occurrence of a stimulus, that had in the experience of experimental subjects been accompanied by the tone, may well have made them quite deliberately and rationally more willing to commit themselves to reporting the tone when they were not quite sure about hearing it. This would result in a lowering of the threshold. Unfortunately, verbal reports are not available.

In other cases, the voluntary response of hand withdrawal was used. Student subjects are notoriously anxious to do the right thing. Karn (1947) asked those of his subjects who responded to the test stimulus why they did so. Two out of 10 could give no explanation. The others gave answers such as, ". . . because I thought it was supposed to." "You must have intended some kind of hook up or you wouldn't have shown them (the two stimuli) together at first." Karn concluded ". . . an association was formed between light and sound during the pre-conditioning period because of an explicit or implicit set on the part of the subject to relate the stimuli to each other." If so, the term *sensory pre-conditioning* is not very appropriate.

Brogden (1947) reported that the majority of subjects in whom he thought sensory pre-conditioning had definitely been established said that they expected shock if they did not respond to the tone and gave as their reason for responding the occurrence of tone and light together in the earlier part of the experiment. The need for using an involuntary response (e.g. pupillary contraction) seems indicated.

Finally, if simple paired presentation of stimuli is to be linked even by name with ordinary conditioning procedures involving an unconditioned response, it should be shown that the same laws govern the outcome in both cases. The strength of a conditioned response is a function of the number of reinforcement trials. Brogden (1950) has summarized two unpublished experiments where an attempt was made to relate the strength of the pairing effect to the number of pairing trials used. No relationship was found, and no relationship is apparent from any of the other experiments quoted. The conditions of extinction suggest other problems. It would appear that presenting one stimulus alone on a large number of trials during training does not result in breaking the connection between it and the stimulus paired with it during pre-training. Extinction of the effect may well take place when responses mediated by it are performed, but the relevant data are available only for two experiments in which the control procedure was unsatisfactory. Ellson (1941), reporting an experiment where repeated pairing of stimuli was shown to be an effective factor in producing auditory hallucinations, concluded that there was no evidence that the hallucinations produced had any of the functional characteristics of conditioned reflexes. In particular, no extinction effects were apparent. Although Ellson continued to use conditioning terminology tentatively, he made it clear that no support for this usage had been obtained experimentally. He was willing to say only that the pairing of stimuli is an effective factor in addition to those present in the older type of suggestion experiment.

Considering the evidence as a whole, Brogden's caution in interpreting his data appears to be entirely justified. In fact we would go a stage further and recommend a change in terminology in order to remove any temptation to premature theorizing. Until more is known about it, the sensory pre-conditioning procedure would be better called *stimulus pairing* and its results *pairing effects*.

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BLINKING DURING VISUAL TRACKING

BY

E. C. POULTON AND R. L. GREGORY

(*From the Medical Research Council Applied Psychology Research Unit, Cambridge*)

In order to determine the incidence of blinks during visual tracking, their effect, and the nature of this effect, two experimental arrangements were used. In one the subject had to keep a pen upon a moving line, his blinks being recorded without his knowledge both electronically and by two observers. In the second he had to keep two pointers in line using a positional control, the display being occluded intermittently in one part by a wheel tachistoscope, in another by his own voluntary blinks.

It was found that the blink rate was raised when the subject expected the tracking to start, and again after tracking. During tracking it was reduced, particularly initially. The lowest blink rates were recorded during or immediately before the difficult periods of the course. It was not possible to predict the blink rate while tracking from a knowledge of the true "resting" blink rate.

Although blinks were infrequent when the course was difficult, it was here that they caused the largest errors, especially when anticipation was not possible. This effect was due partly to interference with vision, but only temporary inattention could account for the delayed deterioration following blinking. It has been suggested that the blink rate might serve as an index of attention, blinking being one of the earliest signs of inattention.

THE PROBLEM

MOST of the previous work upon blinking has been concerned with the effect of various tasks upon the blink rate. They have recently been summarized by Drew (1950). Lawson (1948), however, has produced evidence that blinking can interfere with the performance of tasks requiring constant visual attention, as for example in certain radioactive measurements where small flashes of light are counted. He suggested that blinking may be "a primary cause of proneness to accident" in driving and flying.

The present experiments were primarily concerned with the effect of blinking upon performance. This required elaborate recording, to determine the exact level of performance immediately after blinking; an analysis of the nature of the task, for it was to be expected that the effect of blinking would be more marked when anticipation was not possible; and an investigation into the nature of the effect. The effect might be purely one of interference with vision, as Lawson assumed (1948); or might be due partly to the blinking response competing with the responses required of the subject by the task; or blinking and poor performance might be associated simply because both were associated with inattention. In the latter case the inattention would be the cause of both the blinking and the poor performance, there being no direct causal association between them.

THE METHODS

1. *Main blink experiment*

This was designed and carried out to determine the effect of blinking upon tracking performance. The blinks were recorded both with an electromyograph and by two observers. The electromyograph consisted of an E.E.G.-type direct-coupled amplifier feeding an electromagnetic ink-writing oscillograph. Electrodes were fixed with adhesive tape above and below the left eye, and a third earthed electrode was clipped to the lobe of the left ear. There was no evidence of irritation from the electrodes, and movements were not hampered. The blinks were also recorded on the same paper record by two observers in the next room who watched the subject through peepholes. The subjects were told that their "brain waves" were to be measured. No reference to blinking was made in their presence, and they were unaware of the observers.

The subjects' task was to follow a track with a ball-pointed pen lying in a slot across a horizontal paper tape moving from left to right in front of them at 1 in. per second. The track was a heavy black line drawn in indian ink by a mechanically-driven pen which described simple or complex harmonic courses. The number of harmonic components was changed by switching four motors situated in the next room.

A continuous record was obtained for each subject. On the same paper tape were recorded the following: (1) the harmonic course line, (2) the line drawn by the subject while following the course—the size and nature of his errors could be determined by measuring the differences between these, (3) blinks as recorded by the electromyograph—from this record the temporal position of the blinks in relation to the course could be accurately determined, (4) blinks as observed by the two observers in the next room—these two records ensured that the electronic equipment was recording blinks and nothing but blinks, and (5) a time-marker—this made a special constant-speed control of the tape-driving motor unnecessary.

The subject was unable to see any of the pens, or the records traced by them, excepting only the course line. The horizontal slit along which he moved his pen while following the course was 3·0 mm. wide, and was formed by two perspex bars each 10·0 mm. wide. He had a preview of the course for 10·5 in. right up to his pen. This preview could be screened from him.

Procedure

The various experimental conditions were: (1) an almost straight course to be tracked with 10·5 seconds preview, and (2) the same course with no preview—the course was actually a simple harmonic with a periodicity of 2 cycles per minute and an amplitude of 7·5 mm., (3) a predictable simple-harmonic course (periodicity 25 cycles per minute, amplitude 15.0 mm.) with no preview, (4) a less predictable complex-harmonic course with 10·5 seconds preview, and (5) the same course with no preview. (The complex-harmonic course was produced by two equal-sized harmonic displacements of 21 and 14 cycles per minute, and a 25-cycle per minute displacement of twice this size. Its total amplitude was 30·0 mm.). The five conditions were arranged in Latin Square form with 15 subjects.

The experiment started with a rest period lasting 2·0 minutes, before which the subject was told to relax, "while the machine is being tested out." This was followed by a pause during which the experimenter changed the roll of paper and gave the subject his final instructions, namely that he would have to track continuously for 20 minutes, that the course and the length of view ahead would change from time to time, but that he was not to stop for an instant. This latter point was emphasized, for it was found in pilot experiments that if he was allowed to pause every two or three minutes, his blinking was practically confined to these points.

While he waited, pen in hand, to begin tracking, blinks were recorded for half a minute while the course was still kept covered. Blinks were thus recorded while he was expecting the task to begin. The course was then uncovered, and he received five one-minute practice periods without a break between them. In the test proper, which immediately followed the practice periods, he tracked under each of the five conditions already practised, for periods lasting three minutes, again without a break.

At the end of this 20-minute period of continuous tracking, the experimenter covered the course and told the subject to put down his pen and relax. However, his blinks were recorded for a further half-minute. A pause of about one minute followed; there was then a final two-minute rest period in which blinks were recorded.

Scoring

The total number of blinks scored was 2,400. Each observer missed about 6 per cent. (Neither observer knew when the other pressed his key, for auditory cues were masked by the noise of the course motors, which were situated in the observers' room.) In determining the effect of blinking, only the first eight blinks produced by each subject under each experimental condition were counted. By limiting each subject's contribution, the results of those who blinked relatively infrequently were not swamped by those with high blink rates.

The errors were measured 0·5 seconds before each blink, at the time of the blink, and 0·5 and 1·0 second after it. The errors were classified as "large" when 4·0 mm. or greater, and "small" when less than 4·0 mm. In each case the number of "large" errors was compared with the corresponding figure derived from matched control times.

In determining the exact time of each blink with respect to the course, the complex-harmonic course was divided into half-cycles, extending in each case from an apex to the adjoining trough. These half-cycles were classified according to the amplitude of movement as "large" if greater than 15·0 mm. and "small" if 15·0 mm. or less. Further, each half-cycle of both the simple– and the complex–harmonic course was divided into six equal sections of approximately 0·2 seconds each.

The records were also scored to test two hypotheses as to the nature of the subject's decisions during his blink "blackouts." The first hypothesis was that he ceased initiating all movement during the "blackout." This hypothesis was tested by measuring the error which would have occurred 0·5 seconds after each blink, had he stopped his pen movement one reaction-time (0·25 seconds) after the start of the blink, for the duration of his "blackout" (a further 0·25 seconds). The second hypothesis was that he ceased initiating any new movement during the "blackout," and that he consequently maintained the same speed of movement for a period of 0·25 seconds, beginning one reaction-time after the start of the blink.

2. *Subsidiary blink experiment*

This was to determine the length of time for which the subject had to track before his blink rate rose to a more or less constant level. The experimental layout and procedure were the same as those described above, except for two modifications. First there were two experimenters in the room with the subject and no other observers. One of the two experimenters watched him while he was tracking, and recorded his blinks behind a screen in pencil upon the graphic record. This was the only check upon the electromyograph. There was no suggestion that any subject was aware that his blinks were being recorded while he was tracking.

The second difference was that the same rapid complex-harmonic course, and the same length of preview (0·1 seconds) were used throughout the experiment. The course was produced by three electric motors in the same room as the subject (which gave equal-sized harmonic displacements of 50, 14 and 6 cycles per minute). There were 11 subjects.

3. *Mechanical "blackout" and voluntary-blink experiment*

This was designed to determine the effect upon tracking of interference with vision of about the same duration as the blink "blackout," and to compare this effect with the effect of voluntary blinks. The experimental apparatus was the same as that used in the "blackout" tracking method described by Poulton (1952*b*). The subject had to keep a pointer, operated by a positional control, in line with a "stimulus" pointer which moved in a single vertical dimension in a simple-harmonic or complex-harmonic course. The display was viewed through a screened rotating clear disc. Over this, segments of black paper could be placed, so that the display could be occluded for intermittent short periods.

The mean size of the error when the subject tracked with a clear view of the display was compared with his mean error when the display was "blacked out" about 50 times per minute for 0·05, 0·25, 0·40 and 0·55 seconds respectively. In a further experimental period he had to track while blinking voluntarily at the same rate per minute. (These high rates were chosen in order to exaggerate the effects.) The experiment was divided into two halves. Half the subjects started with the simple-harmonic course (of 25 cycles per minute). The rest started with the complex-harmonic course (produced by three equal-sized harmonic displacements of 14, 22·5 and 25 cycles per minute). The six experimental periods in each half were arranged in Latin Square form, with 12 subjects. Each period lasted one minute, but only the second half of it was scored.

Practice periods were given before each half. From these the rate of voluntary blinking was determined. The mechanical "blackout" rate was equated with it, except in cases where it corresponded closely to the periodicity of the simple-harmonic course, when a slightly lower or higher rate was chosen. The subjects averaged 24·9 blinks in the second half-minute of the experimental blinking period with the simple-harmonic course, as against a mean mechanical "blackout" rate of 25·0. With the complex-harmonic course they averaged 23·0 blinks as against a mean 23·3. The greatest individual discrepancy between blink rate and "blackout" rate per half-minute was seven.

4. *Subsidiary voluntary-blink experiment*

This was carried out upon five comparable subjects, in order to relate the length of voluntary- and normal-blink "blackouts." None of the subjects knew the aim of the experiment, which was performed out of doors. One or two 30-second close-up ciné shots were taken of each of them separately, depending upon his rate of blinking, while the experimenter and photographer looked away and conversed. The subject looked into the near distance.

The subject was then given the same instructions about voluntary blinking as the subjects in the tracking experiment, and told to practice a few voluntary blinks. A further 30-second close-up ciné shot followed, while he blinked about once every 1·5 seconds, in time with the experimenter's conducting. The ciné camera was set at 16 frames-per-second throughout. The average number of frames per blink in which the experimenter judged that vision was completely obscured by the eyelids was then calculated for the normal- and voluntary-blink periods and compared.

THE INCIDENCE OF BLINKS

Expectancy

The mean number of blinks per minute before, during and after tracking in the main blink experiment was as follows:—

"Resting" before experiment	17·6
Immediately before tracking	23·5
Tracking straight course	8·6
Tracking curved course	2·7
Immediately after tracking	41·6
"Resting" after experiment	21·6

All the means are significantly different at the 5-per-cent. point, some at the 0·1-per-cent. point.

The blink rate immediately before tracking was determined under conditions identical with the "resting" blink rate, except that the subject held a pen in his hand, and knew he was about to start the experiment. Under these conditions the mean blink rate rose by about one-third.

It seems likely that many people show this more or less involuntary increase in blink rate when they are about to start a fairly arduous unfamiliar task. Changes in blink rate may thus give some indication of "anticipatory tension."

Initial "shock"

In the subsidiary blink experiment the mean number of blinks per minute for the first minute of tracking was 0·6. For the next three minutes it averaged 2·2, and from the fifth minute onward it averaged 3·3. These differences are significant at the 5-per-cent. point.

The initial low blink rate corresponds to the "initial spurt" of many laboratory work-curves. A similar inverse association between blink rate and level of work as shown by the number of missed signals in successive half-hour periods of the Clock Test, will be seen to be present when a comparison is made between the work of Carpenter (1948) and Mackworth (1950), for the blink rate was relatively low in the first half-hour, when few signals are usually missed, and higher in the succeeding half-hours, when more signals are missed. Yet blinks never apparently coincided with missed signals. It seems likely therefore that the blink rate is inversely associated with the degree of "concentration" or "attention." It may be possible to use it as an index of attention.

Level of work

It will be seen from the results reported above that as compared with the "resting" blink rate before the experiment, the blink rate was halved while tracking an almost straight-line course. It was further reduced by a factor of 3 while tracking a curved course. Drew's (1950) findings were in general similar. This suggests that, in estimating the amount of time for which vision is obscured in visual tasks, it is misleading to use "resting" blink rates, as has been done by Lawson (1948). In the present instance such a procedure would clearly be quite invalid.

It was also found that when the initial "resting" blink rate of each subject was compared with his mean tracking blink rate, the product-moment correlation coefficient was only +·13. This shows the small value of each subject's true "resting" blink rate in predicting how much he will blink while tracking.

The corresponding correlation coefficient calculated from Drew's results was +·73. This high value was associated with a rather low "resting" blink rate, as compared with that found by us. This was probably because in Drew's experiment the "resting" blink rate was estimated by an experimenter in the same room. We found that when observed under such conditions, subjects tended to fixate objects for rather longer than they would have done otherwise, and so to blink rather less frequently. Some constraint was clearly being placed upon them. The "resting" and tracking conditions were thus not so different. Drew's results suggest that a better prediction of the tracking blink rate could be made from the "resting" blink rate if measured while subjects were aware that they were being observed, although not, of course, aware that blink rate was being measured.

Temporary restraint

With the complex-harmonic course 62 per cent. of the blinks occurred during small amplitude half-cycles and only 38 per cent. during large. The expected values were 48 and 52 per cent. respectively, the difference being highly significant. Further, for all three conditions of curved courses combined, 42 per cent. of blinks occurred during the 0·4 seconds immediately preceding the apices and troughs (the points of zero speed of displacement) and only 25 per cent. during the 0·4 seconds immediately following them. The expected frequency was in both cases 33 per cent., the difference being again highly significant.

Clearly subjects concentrated their blinks during or immediately before the easy parts of the course and tended to avoid blinking during or immediately before the more difficult parts.

Recovery

The mean blink rate immediately after tracking was two and one-third times as great as the "resting" blink rate before the experiment. Over two minutes later the blink rate was still slightly but significantly raised.

The blink rates of the individual subjects after tracking correlated about +·80 by the product-moment method with their initial resting rates. (Any value above +0·51 shows a significant correlation at the 5 per cent. level of significance.) Further, estimates of their "blink deficits" accumulated while tracking correlated only about +·10 with estimates of the rate at which these deficits were made good after tracking had been completed. Thus after tracking subjects merely showed an exaggeration of their initial "resting" blink rate pattern. There was no straightforward relationship to the degree of blink deprivation.

THE EFFECT OF BLINKS

Nature of task

With the almost straight-line courses blinking had no effect upon tracking. This was so both when the subject was given a preview of the course of 10·5 seconds, and when he had no preview. However, with the curved courses, blinking did affect tracking. Fig. 1 shows the distributions of errors with the curved courses 0·5 seconds after blinking.

There was no difference between the error distribution 0·5 seconds before blinking and the control distributions, nor between the distributions at the time of blinking and the controls. Further, by 1·0 seconds after blinking the distributions of errors had returned to the control form, except in the case of the complex-harmonic course without preview, where there was still a predominance of errors of 6·0 mm. and greater, which was significant at the 5 per cent. point.

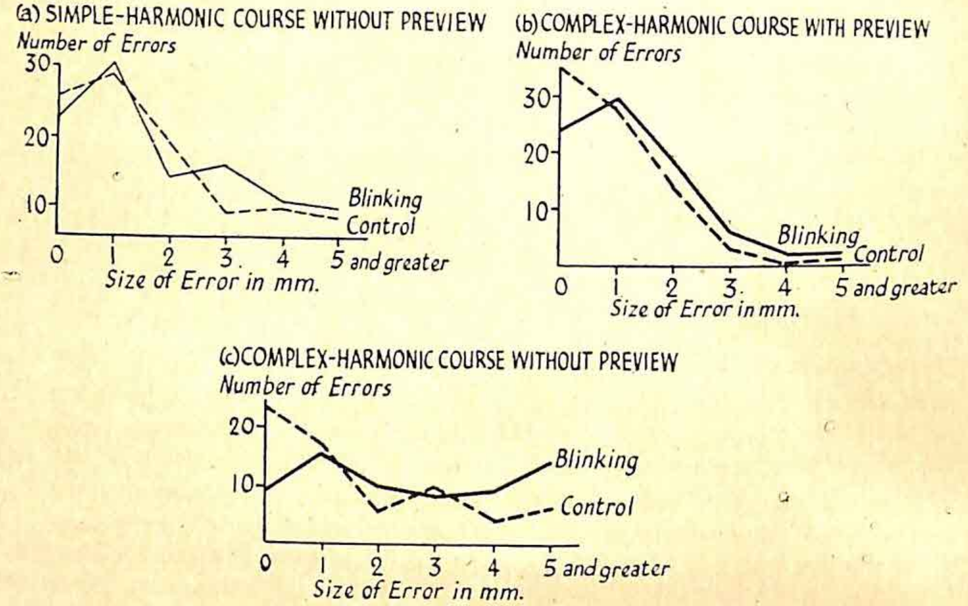

FIG. 1. Errors following blinking grouped according to size. (a) Simple-harmonic course without preview. (b) Complex-harmonic course with preview. (c) Complex-harmonic course without preview. Continuous lines: errors 0·5 seconds after blinks. Broken lines: errors at comparable control times. The amplitude of movement of the simple-harmonic course was 15·0 mm., the total amplitude of the complex course 30·0 mm. The distributions in (a) and (b) are not significantly different. However those in (c) are significantly different at well beyond the 1 per cent. point.

Under all three conditions of curved courses, there were rather fewer small errors, and rather more large errors, 0·5 seconds after blinking than at the control times. However the difference was only significant for the complex harmonic course without preview, when anticipation was reduced to a minimum. Where the subject was allowed a preview, and where the course was simple harmonic and consequently completely predictable, anticipation was possible (Poulton, 1952*a*). It was only with the complex-harmonic course without preview that the subject had little idea of what the course was about to do next.

Several conclusions follow from these results. Blinking can have a detrimental effect upon a task such as tracking. However, it has no effect when the course is almost a straight line. It has a detrimental effect only upon a proportion of occasions even when the course is curved. And this effect is considerably reduced when either kind of anticipation is possible.

Timing of blinks

When the subject did not blink, he performed significantly less accurately during the 0·4 second period covering the point of inflexion of the course than during the 0·4 second periods on either side of the apex or trough. The criterion used in obtaining this result was the number of large errors (4 mm. or greater) found in samples from all three conditions of curved courses combined. The result corresponds to the finding of previous experiments that within reasonable limits the tracking error varied directly with the speed of displacement (Poulton, 1952*a*), for the maximum speed of displacement occurs at the point of inflexion.

In contrast, 0·5 seconds after blinking the subject was found to be performing least accurately during the 0·4 second period immediately before the apex or the trough. Large errors following blinks were thus distributed in a significantly different way from those at the control times. When he did not blink, the 0·2 second period of the course showing the highest number of large errors was the period following the point of inflexion. Whereas when he blinked it was the 0·2 second period immediately beyond this.

This was to have been expected. The short but most difficult section of course on either side of the point of inflexion will normally result in a maximal error about one reaction time later, i.e. just before the subject is able to make a correction. Whereas a blink, depriving him of vital information at such a time, will delay his correction for a further period corresponding to the duration of his "blackout," so that his maximal error will occur this much later. Thus the distribution of large errors following blinking will have its maximum rather later than the control distribution of large errors.

THE NATURE OF THE EFFECT

Interference with vision

The mean tracking error with different lengths of mechanical "blackouts" is shown in Table I, which shows also the effect of voluntary blinking at the same average rate per minute. It will be seen that the greater the length of the "blackouts," the greater was the deterioration in performance with the complex-harmonic course. With the simple-harmonic course no deterioration was shown with mechanical "blackouts," in this experiment. However, in a previous experiment on 12 subjects, a significant deterioration was found with this course also, although it was significantly less than with the complex-harmonic course.

TABLE I

EFFECT OF MECHANICAL "BLACKOUTS" AND VOLUNTARY BLINKS WHILE TRACKING

		Mean error for	
		Simple-harmonic course	*Complex-harmonic course*
Duration of mechanical "blackouts" (in seconds)	0·0	23	26
,,	0·05	19	24
,,	0·25	25	31
,,	0·40	22	35
,,	0·55	23	45
Voluntary blinks	—	32	39

For 5-per-cent. significance, differences must be about 5. The level of error which would have occurred if the subject had not responded at all has been taken as 100.

These results with mechanical "blackouts" correspond to the results following blinks described above. Both blinks and mechanical "blackouts" of the same length produced deterioration in tracking moving courses. But in both cases the effect was less with simple-harmonic courses, which allow perceptual anticipation. This strongly suggests that at least part of the deterioration in tracking associated with blinking is due to interference with vision.

Competition between responses

Table I also shows the mean tracking error in the second part of the experiment, when subjects blinked voluntarily at the same rate per minute as the mechanical "blackout" rate. It will be noted that these results are for voluntary blinks. Close-up ciné photographs suggested that in most subjects the voluntary blink "blackout" is about 20 per cent. longer than the normal "blackout," a difference significant at the 5-per-cent. point.

With the simple-harmonic course the mean error with voluntary blinks was significantly greater than the error with mechanical "blackouts" of 0·55 seconds. Yet this is the maximum average duration of the interference with vision caused by normal blinking (the sum of "blackout" time and "mobile vision" time—Lawson, 1948). The difference does not appear to be explainable simply upon the grounds that voluntary-blink "blackouts" are generally of about 20 per cent. longer duration than normal-blink "blackouts."

It seems likely therefore that voluntary blinks can have an additional detrimental effect upon performance. This is presumably because attention is distracted from the task. Where normal blinking is a nervous habit, it may also have this distracting effect.

Inattention

As described above, the complex-harmonic course without preview, the experimental condition which showed the most marked deterioration 0·5 seconds after blinking, still showed a significant deterioration 1·0 second after blinking. This was due to 15 per cent. of errors of 6 mm. and greater. All these very large errors actually increased in size between 0·5 and 1·0 second after blinking.

On the assumption that the detrimental effect of blinking was due to interference with vision, and possibly competition between responses, it is to be expected that by 0·5 seconds after blinking the subjects would have been starting to correct the misalignment caused by blinking; and that by 1·0 second after blinking much of this correction would have occurred. That in these cases the error actually increased in size during this time suggests that blinking was accompanied by a lapse of attention.

This lapse of attention may in fact have been in some sense the cause of the blinking. However, there was no significant increase in error between 0·5 seconds before blinks and the times of the blinks themselves, as would be expected if attention had lapsed sometime before blinking occurred. It follows that in a task such as tracking, blinking may be the earliest sign of inattention.

On this view, the significant deterioration in tracking found 0·5 seconds after blinking could have been caused partly by the blinking, and partly by the inattention of which blinking was the earliest sign. The sequence of events would thus be as follows:—

↗Blinking ——> Short term deterioration
Inattention
↘Short term and more prolonged deterioration

PERFORMANCE WHILE BLINKING

In order to throw light upon the nature of the subject's decisions during normal blink "blackouts," theoretical errors were estimated for the three conditions under which he tracked curved courses, on two assumptions. These assumptions were: (1) that the subject ceased initiating all movement during the "blackout" period, and (2) that he ceased initiating any new movement during this period, maintaining instead his initial speed of movement. In all cases the theoretical errors were much greater than the subject's actual errors. The constant-speed-of-movement hypothesis gave the less inaccurate approximation.

It seems clear that the subject must have been responding to an acceleration component of the course during his blink "blackouts," as well as to a velocity component. He must in fact have been tracking an expected course during his "blackouts."

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THE SENSIBILITY TO PASSIVE MOVEMENT OF THE HUMAN ELBOW JOINT

BY

T. E. CLEGHORN* AND H. D. DARCUS

(From the Unit for Research on Climate and Working Efficiency of the Medical Research Council, Department of Anatomy, University of Oxford)

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The literature on the perception of passive movement is reviewed. A description is given of an apparatus whereby passive movements may be produced at the human elbow joint with the minimal amount of interference with sensation from extraneous factors. Tested with this apparatus, four human subjects experienced in varying degree false sensations of movement when the arm was at rest. They differed from each other in the direction which they tended to ascribe to the sensations, three of them being strongly influenced by the direction of the preceding movement.

Both the perception of movement and identification of its direction were more efficient in extension than in flexion and at larger than at smaller displacements. The duration of movement was an important factor. Tentative estimates of the threshold are: 0·8° for 80 per cent. correct detection of movement and 1·8° for 80 per cent. correct identification of direction at all speeds in the range, 0·10–0·25°/sec.

I

INTRODUCTION

The sensations aroused by passive movements of the joints have hitherto received very little attention. In the few physiological studies which are available, investigators have sought to establish normal values for the amount of angular displacement which must occur at a joint before a sensation of movement is aroused. Modification of the threshold values so obtained has been brought about by means of various anaesthetic procedures in an attempt to establish the precise location of the sensory receptors.

General review of previous work. Goldscheider (1889) used himself as a subject and investigated many of the joints of the left side of his body. He noted that, in general, threshold values were smaller where the skeletal elements of the joint were longer; for example, they were smaller in the shoulder and hip joints than in those of the hand and fingers, with the elbow joint occupying an intermediate position. He found that anaesthesia to the region of the elbow joint induced by a faradic current brought about a considerable blunting of sensibility of passive joint movement, and this led him to postulate that the receptors concerned were located within the limits of the joint capsule. He claimed to obtain support for this hypothesis from the results of a number of experiments on rabbits and frogs, in which he carried out various manipulations within the joint capsule and assessed his results by changes in respiratory rate.

Pillsbury (1901) confined his studies to the elbow and knee joints of three subjects. His values for the threshold of movement were of the same order as those of Goldscheider, and he confirmed the effects of faradisation. He noted, however, that sensibility was similarly blunted when the faradic current was applied at a point distal to the joint under investigation, which suggested to him that the sensation of movement was derived from tendons and muscles rather than from the articular structures themselves.

Winter (1912) extended these observations, using seven subjects, including Pillsbury and himself, and reached a similar conclusion. Movements were carried out at four different speeds in flexion and extension from various initial positions of the elbow joint. He stated that the subjects referred the sensation to the muscles and tendons when cutaneous stimulation was eliminated by the application of the faradic current or by ether swabs to the skin.

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* Seconded from the Directorate of Physiological and Biological Research, Ministry of Supply.

Laidlaw and Hamilton (1937*a*, *b*) studied a number of joints on both sides of the body in sixty subjects in order to establish a normal range of the threshold of passive movement for comparison with clinical cases. They noted no difference between the two sides of the body and were unable to demonstrate any influence of age, sex, occupation or educational standards.

Goldscheider, Winter, and Laidlaw and Hamilton all draw attention to the extreme variability of the results obtained with the same subject on different occasions and with different subjects. However, these variations are obscured by the presentation of the results as mean values.

Winter noted that a subject, when expecting a movement to take place, would frequently "detect" a movement when none had actually occurred. This effect does not appear to have been studied systematically, although, if it was at all pronounced, the threshold values previously recorded must be regarded with some reserve.

The present study was undertaken to determine some of the factors which may influence the detection of passive movement of the elbow joint and to assess the relative importance of the phenomenon of false sensation in studies of this nature.

Apparatus and procedure used in previous studies. Goldscheider (1889) supported his forearm in a plush-lined wooden trough which was moved vertically by a hydraulic system with a master pump in the basement of the laboratory. He was in communication with the operator by a speaking tube. The forearm was enclosed in a tightly fitting double-walled rubber sleeve filled with water, in an attempt to avoid local pressure from the apparatus. Each movement was recorded on a moving smoked drum. From this record, the speed and amplitude of the angular displacement were calculated. A scale of nine arbitrary degrees of sensation was constructed and the sensation of each movement was assigned to one of these. Each experimental series consisted of about thirty movements, and the magnitude of successive displacements was increased in order to approach a point when a true sensation of movement was just appreciated. The displacement which was "just perceived" on half or more than half of the occasions was taken as the threshold value. Neither the speed nor amplitude of movement was under complete control and they appear to have varied independently in rather a haphazard manner. The interval between each movement is not stated.

TABLE I

A SUMMARY OF THE DATA OBTAINED IN PREVIOUS STUDIES OF THE THRESHOLD FOR PASSIVE MOVEMENT AT THE HUMAN ELBOW JOINT

Investigator	*Subjects*	*Trials per subject*	*Range of speed (deg./sec.)*	*Threshold range (deg.)*
Goldscheider	1	334	0·7–1·4	0·40–0·76
Pillsbury	3	153*	0·33	0·43–0·85
Winter	7 (2)	135*	0·08–0·56	0·20–2·82
Laidlaw and Hamilton . .	60 (20)	10	0·16	0·30–2·5

Figures in parentheses indicate number of subjects for whom incomplete data is presented.
* = average values.

Pillsbury (1901) used a much simpler technique and allowed the forearm of his subject to rest on a hinged board with no fixation of any kind. Flexion or extension movement, which was in the vertical plane about a transverse axis located below that of the joint, was produced by a constant speed electric motor working through a system of pulleys. The motor was allowed to run continuously throughout the experimental series, movement being initiated and terminated by means of a clutch mechanism. The extent of the movement was registered by a pointer moving over a vertical scale. The signal that the movement had started was given to the subject when the board was seen to move, and the subject indicated when the movement had been appreciated. The speed of movement was 0·3°/sec. Winter (1912) used the same apparatus but employed a range of four speeds and allowed an interval of two seconds between warning the subject that a movement was about to start and the start of the movement. In neither instance is the interval between successive movements stated.

Laidlaw and Hamilton (1937*a*, *b*) used a method similar to that of the previous two workers, but flexed or extended the elbow joint in the horizontal plane about a vertical axis. Movements were at a constant speed of 0·16°/sec. and were produced by a hand crank operated in time with the beat of a metronome. A number of joints on both sides of the body were tested in the series; five movements being made in each direction for each joint on either side. No further details are given of the procedure, except that the directions of the movement were presented in a random order.

In not one of these three studies, using the direct method, does any allowance appear to have been made for the time lag which must have occurred between the instant when the sensation of movement was actually experienced and when the observer recorded the reading of the pointer on the scale. Considerable distortion of results must have resulted from this delay, particularly at the higher speeds, and the precision of the measurements appears to be out of all proportion to the probable accuracy of the method.

II

METHODS

Apparatus used in the present study. A trough of light alloy pivoted on ball bearings and moving in the vertical plane was constructed to support the forearm. This trough could be adjusted so as to commence movement at any desired angle from the horizontal to 45° from that position. A vertical driving rod, working through lubricated guides, came into contact at its upper end on the free end of the trough and was linked at its lower end with a horizontal rod. This passed through an adjustable fulcrum and connected at its other end with a cam driven by a constant speed electric motor. Friction between the driving surfaces was reduced to a minimum by the use of ball races, and close contact was maintained by means of springs. The electric motor ran continuously throughout an experimental series and the cam was thrown in and out of gear by means of a clutch mechanism. A movement was started by engaging the gears by hand and ended at a selected time by an adjustable time switch on the cam, which automatically threw the motor out of gear. By suitable adjustment of the sliding fulcrum and of the time switch, it was possible to produce movements at any desired speed up to a maximum of 0·5°/sec. and for durations of a maximum of 40 seconds. The cam was so constructed as to run level for four seconds before driving commenced. Movement in the opposite direction was produced by reversal of the cam—a simple and silent procedure. A rigid trough was attached to the apparatus to accommodate the upper arm. This trough was made sufficiently wide to allow for differences in the carrying angle in various subjects. Webbing slings were fixed in both troughs, leaving free the region 3 in. on either side of the pivots.

Fixation of the upper limb to the apparatus. Difficulties were encountered in fixing the limb to the apparatus, owing to changes in contour of the limb on movement. The most suitable method was to wrap the forearm and hand with fingers extended in a thick layer of cotton wool extending from the finger tips to a point 2–3 in. distal to the transverse axis of the elbow joint. The cotton wool was held in position by a cotton gauze bandage. This was then enclosed in a double layer of thin lead sheeting, leaving a cuff of wool uncovered near the elbow joint. The arm was covered in a similar manner. The limb was then placed in the troughs, with the transverse axis of the elbow joint coincident with the axis of the apparatus, and secured in place by rubber straps under a slight degree of tension.

Design of experiment. After extensive trials, it was concluded that Goldscheider's approach to the problem presented fewer technical difficulties than did the more simple threshold methods employed by other workers. It was thus decided to present to the subject a series of movements, each of a different speed, amplitude and direction, and by suitable replication of the series to determine the proportion of movements detected successfully in each category. The analysis of sensation employed by Goldscheider was considered to be too elaborate, and the subject was only required to state whether a particular movement had been appreciated, and if so, in which direction it had taken place. In order that information might be obtained regarding the false sensations of movement noted by Winter, a number of "nil movements" were introduced into each series. These were initiated and concluded in the manner described for the true movements and differed from these only in that the driving rod was disconnected from the cam so that no actual movement took place. Each was of ten seconds duration. The subjects were unaware of the interpolation of these "nil movements" in the series and were told only that movement would occur but might or might not be appreciated. Amplitudes of displacement of 0·5°, 1·0° and 2·0° were selected as covering the greater part of the range of threshold values obtained by other workers. These displacements were presented in both flexion and extension directions at each of five speeds, 0·05°, 0·10°, 0·15°, 0·20° and 0·25° per second, making a total of thirty movements in each experimental series. The series was completed by the addition of ten "nil movements." The order of presentation was random. Four subjects were selected and each submitted to thirty series of experiments, thus obtaining a score out of thirty for each displacement/speed/direction combination and a score out of 300 for the "nil movements."

Experiments were conducted in a quiet room with the subject in a comfortable supine position and blindfolded. The shoulder joint was held at 90° of abduction and the initial position of the elbow joint was at 30° in the vertical plane from full extension with the plane of the hand vertical. A resting period of five minutes was given after fixation in the apparatus before the experiment was started. Intervals of thirty seconds were allowed between successive movements. In most instances, only one series was carried out with any particular subject on one day. On the few occasions when exceptions to this rule became necessary, an interval of four hours was allowed between series, and a maximum of two series per day was never exceeded. The signal to the subject that the movement was about to occur was the click of the clutch as the motor was thrown into gear. Movement actually started four seconds later. The end of the movement was indicated by the click of the motor being thrown out of gear. At this point, the subjects were required to make their decisions.

Subjects. Two subjects (M and C) were naval ratings attached to the Unit for experimental purposes. T was a carpenter by trade, while D was a secretary, both members of the Unit. D was the only female in the study. All had been employed in other studies on sensation and had had numerous trials with the apparatus before recording was started.

Treatment of results. The results have been divided into the response to "nil movements" and the response to true movements. In assessing the results of the appreciation of true movements, two levels of success have been used. The first is the ability to detect movement irrespective of direction and the second the ability to ascribe the correct direction to the movement. Before subjecting these figures to a four-factor analysis of variance, they have been converted to proportions and then to angular values by the method of Fisher and Yates (1948). To simplify the presentation of the results, only the raw data have been included in the tables. The level of significance attained by the main variables and their interactions of the first order are summarized in Table VII.

III

RESULTS

"*Nil movements*" (*Table II*)

Proportion of "nil movements" correctly reported. Averaging the results of the four subjects, "nil movements" were correctly reported on 52 per cent. occasions, but there was a wide individual variation.

Direction ascribed to "movement" reported in "nil movements." In the overall results of the runs in which movement was said to have occurred, although in fact it had not, the movement was thought to be in the direction of extension more often than flexion. The same was true of three of the subjects (M, C and T), but in only one (T) was this statistically significant. In the fourth subject (D), flexion was chosen a significantly greater number of times than extension.

Influence of the preceding movement on the successful detection of "nil movements." With subjects M and C, a greater proportion of "nil movements" were recorded correctly when preceded by another "nil movement." This difference was not noted

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TABLE II

THE INFLUENCE OF THE PRECEDING MOVEMENT ON THE SCORING OF 300 "NIL MOVEMENTS" BY EACH OF FOUR SUBJECTS

Subject	*Scored as*	*Nature of preceding movement*				*Percentage correct results*
		Flex.	*Ext.*	*"Nil"*	*Total*	
M	Flex. Ext. "Nil"	22 10 *66*	5 31 *65*	12 5 *84*	39 46 *215*	72%
	Total	98	101	101	300	
C	Flex. Ext. "Nil"	32 22 *44*	20 35 *34*	21 23 *69*	73 80 *147*	49%
	Total	98	89	113	300	
T	Flex. Ext. "Nil"	6 34 *56*	9 43 *58*	10 31 *53*	25 108 *167*	56%
	Total	96	110	94	300	
D	Flex. Ext. "Nil"	62 18 *29*	37 41 *34*	31 19 *29*	130 78 *92*	·31%
	Total	109	112	79	300	

The italic figures indicate the number of correct results.

TABLE III

THE SCORES BY EACH OF THE FOUR SUBJECTS (A) AT THE THREE DISPLACEMENTS (MAXIMUM SCORE = 300) (B) AT THE FIVE SPEEDS (MAXIMUM SCORE = 180)

Displacements (degrees)	*Movement detected*					*Direction identified*				
	Subjects					*Subjects*				
	M	*C*	*T*	*D*	*Total*	*M*	*C*	*T*	*D*	*Total*
A. ½	228	199	206	221	854 (71%)	168	145	177	127	617 (51%)
1	253	242	249	260	1,004 (84%)	208	196	239	163	806 (67%)
2	*277*	*274*	*279*	*277*	*1,107* (92%)	*251*	*251*	*273*	*207*	*982* (82%)
Total	758 (84%	715 79%	734 82%	758 84%)	2,965 (82%)	627 (70%	592 66%	689 77%	497 55%)	2,405 (67%)
Speed (deg./sec.)										
B. 0·05	109	136	141	158	544 (76%)	100	114	128	*119*	461 (64%)
0·10	149	149	140	*162*	600 (83%)	132	*128*	133	106	*499* (69%)
0·15	163	*151*	*152*	145	611 (85%)	*139*	125	141	87	492 (68%)
0·20	*171*	144	*152*	147	*614* (85%)	133	116	*147*	90	486 (67%)
0·25	166	135	149	146	596 (83%)	123	109	140	95	467 (65%)

The italic figures indicate the maximum scores by each subject. The figures in parentheses are the correct results expressed as percentages.

in the other subjects. With none of the subjects, when the "nil movement" was preceded by a true movement, did direction of this influence the ability to detect a "nil movement" correctly.

Influence of the nature of the preceding movement on the direction ascribed to "nil movements" in which movement was detected. With three of the subjects (M, C and D), the direction of the preceding movement had a highly significant influence on the direction ascribed to a "nil movement" when a sensation of movement was experienced. In these subjects, the direction in which the "movement" was thought to occur was more often the same as that of the preceding true movement. With subject T, there was no influence of the preceding movement on the direction ascribed to the "movement" detected in the "nil movement."

When a sensation of movement was experienced in a "nil movement" which followed another "nil movement," subjects M and D favoured flexion in the ratio of 2 : 1 and 3 : 2 respectively. T, on the other hand, favoured extension in the ratio of 3 : 1—a bias of an order similar to his impressions of the direction of "nil movement" following a true movement. Subject C showed no preference, judging the supposed movement to be either flexion or extension in almost an equal number of times.

True movements

The detection and identification of the direction of movement.

The detection of movement. Of the 900 movements presented to each of the four subjects, 82 per cent. were detected (Table IIIA). The scores of C and T differ significantly from each other and from M and D.

TABLE IV

THE SCORES AT THE THREE DISPLACEMENTS AT THE FIVE SPEEDS (MAXIMUM SCORE = 240)

	Movement detected			*Direction identified*		
	Displacement			*Displacement*		
Speed (deg./sec.)	½°	1°	2°	½°	1°	2°
0·05	166	180	198	131	152	178
0·10	*179*	202	219	*137*	*168*	194
0·15	169	*213*	229	122	*168*	202
0·20	175	210	229	124	163	199
0·25	165	199	*232*	103	155	*209*

The italic figures indicate the maximum scores at each displacement.

The ability of each subject to detect a movement improved rapidly as the degree of displacement increased (Table IIIA). The improvement was largest between ½° and 1° displacement and least between 1° and 2°. These changes are highly significant.

More movements were appreciated as the speed was raised from 0·05° to 0·20° per second, with a maximum rise between 0·05° and 0·10° per second (Table IIIB). Between speeds of 0·20° and 0·25° per second, there was a falling off in the number of movements detected. These differences, with the exception of that between the scores at 0·15° and 0·20° per second, are of statistical significance. The pattern varied from subject to subject, but, although the individual trends are clearly defined, the differences are not statistically significant.

The detection of movement of ½° displacement was greatest at 0·10° per second and that of 1° displacement at 0·15° per second (Table IV). At each of these displacements, there was a rise and fall to and from the peak at the lower and higher speeds. At displacements of 2°, on the other hand, there was a steady increase in the number of movements detected with increasing speeds.

The identification of direction. The direction was identified correctly in 67 per cent. of the total number of movements (Table IIIA). The individual differences which occurred are highly significant.

With increasing displacement, the proficiency in identifying the direction improved significantly even more rapidly than that in detecting movements, but the tendency to level off between 1° and 2° was not so marked (Table IIIA). The same order of success was achieved by each subject.

The influence of speed showed the same general pattern as in detecting movement, although the peak performance was reached earlier at 0·10° per second instead of 0·20° per second (Table IIIB). The differences between the scores at the different speeds do not reach statistical significance. Each subject reached the peak at a different speed. When these results are compared with the detection of movement, it can be seen that M, C and D attained their best performance at one speed slower and T at one speed faster. Unlike the results for the detection of movement, the interactions reach a statistical level of significance.

The changes observed in the detection of movement at different speeds and displacements were paralleled in the identification of its direction, although they were less pronounced (Table IV).

The effect of the direction of the movement.

The detection of movement. Of the 1,800 movements in each direction, movements were detected significantly more often when they were in the direction of extension than of flexion; extension movements were appreciated on 85 per cent. occasions as compared with 80 per cent. for flexion (Table V). Each subject showed the same effect, although to a varying degree.

TABLE V

THE SCORES BY EACH OF THE SUBJECTS IN FLEXION AND EXTENSION (MAXIMUM SCORE = 450)

Subjects	*Movement detected*			*Direction identified*		
	Flex.	*Ext.*	*Difference (per cent.)*	*Flex.*	*Ext.*	*Difference (per cent.)*
M	374	*384*	2·2	266	*361*	22·2
C	349	*366*	3·8	269	*323*	12·0
T	342	*392*	11·1	303	*386*	18·4
D	378	*380*	0·4	241	*256*	3·3
Total	1,443 (80%)	1,522 (85%)	4·4	1,079 (60%)	1,326 (74%)	

The italic figures indicate the maximum scores by each subject. The figures in parentheses are the correct results expressed as percentages.

The amplitude of the displacement affects the tendency to detect extension movements more often than flexion (Table VIA). The greatest difference was at a displacement of 1° and the least at one of ½°.

The influence of direction decreased with speeds up to 0·15° per second until at the succeeding speed (0·20°/sec.) the preference was reversed to a small degree, a change which was slightly intensified at the highest speed (0·25°/sec.) (Table VIB). These differences, however, are not significant at the higher speeds.

TABLE VI

THE SCORES IN FLEXION AND EXTENSION AT (A) THE THREE DISPLACEMENTS (MAXIMUM SCORE = 600) AND AT (B) THE FIVE SPEEDS (MAXIMUM SCORE = 360)

	Movement detected				*Direction identified*			
Displacement (degrees)	*Flex.*	*Ext.*	*Total*	*Difference (per cent.)*	*Flex.*	*Ext.*	*Total*	*Difference (per cent.)*
A. ½	*428*	426	854	−0·3	263	*354*	617	15·2
1	473	*531*	1,004	9·7	352	*454*	806	17·0
2	542	*565*	1,107	3·8	464	*518*	982	9·0
Speed (deg./sec.)								
B. 0·05	253	*291*	544	10·6	199	*262*	461	17·5
0·10	280	*320*	600	11·1	217	*282*	499	18·3
0·15	300	311	611	3·0	224	*268*	492	12·2
0·20	*309*	305	*614*	−1·1	228	*258*	486	8·3
0·25	*301*	295	596	−1·7	211	*256*	467	12·5

The italic figures indicate the maximum score at each displacement or each speed.

The identification of the direction. The difference between the results for extension and flexion movements noted in the detection of movement was considerably increased when the identification of its direction was taken into account (Table V). The average scores in this instance are 74 per cent. correct answers for extension movements and 60 per cent. for flexion movements; a difference which is statistically highly significant. The increased effect was shown by each of the subjects and only in D did it fail to reach statistical significance.

The same relative success occurs at all displacements (Table VIA). Although with increasing speeds the difference between extension and flexion tends to decrease, there is no reversal of the effect as found in the detection of movement (Table VIB). There is no significant interaction between direction and speed nor direction and displacement.

The sensation experienced. Although the present study was planned deliberately to reduce introspection by the subject to a minimum, certain unsolicited observations were made by the subjects on the sensations they experienced. All the subjects were agreed that the sensation was hard to define, owing to the difficulty of isolating it from the numerous extraneous sensations introduced by the apparatus itself. They also stated that the movement was often sensed suddenly and strongly, only to fade away completely and then to return once more. This waxing and waning process might be repeated several times in the course of the same movement. On certain occasions, they even declared that the movement had altered direction during one of these episodes; in this event, they were instructed to define the direction which they sensed most strongly.

They all commented on the fact that sensation of movement was seldom experienced in the region of the elbow joint itself but was usually referred to a point lower down the forearm, most often the hand. No apparent distinction was made between true movements and "nil movements" in this respect.

TABLE VII

SUMMARY TABLE TO ILLUSTRATE THE LEVEL OF STATISTICAL SIGNIFICANCE ATTAINED BY THE MAIN VARIABLES AND THEIR INTERACTIONS OF THE FIRST ORDER, IN THE DETECTION OF MOVEMENT AND IN THE IDENTIFICATION OF ITS DIRECTION

	Movement detected	*Direction identified*
SUBJECTS	XXX	XXX
DIRECTIONS	XXX	XXX
DISPLACEMENTS	XXX	XXX
SPEEDS	XXX	—
Subjects and Directions	XX	XXX
Subjects and Speeds	—	XX
Subjects and Displacements	—	—
Directions and Speeds	XX	—
Directions and Displacements	XX	—
Displacements and Speeds	XX	XX

XXX significant at 0·1% level.
XX significant at 1·0% level.
— not significant.

IV

DISCUSSION

"Nil movements"

It is clear that the sensation arising in the course of the "nil movements" differs in no demonstrable way from that aroused by the true movements. It has been difficult, however, to relate the findings in the two cases. Several attempts have been made to relate the scores obtained with the true movements with the results of the "nil movements." It is felt that the latter can in no way be regarded as a true indication of the guess rate for any particular subject. It is true that one can "correct" scores in the true movements by means of a factor derived from the scores of the "nil movements." Such manipulation, however, in no way affects the general trends described. In fact, although it lowers the success achieved, it increases the differences between the scores at the various speeds and amplitudes of movement. One feels that until more is known of the nature of the "nil" sensation, it would be preferable to leave the results as they stand. The correlation of the direction of the preceding movement with the direction ascribed to "nil movements" in which movement was detected suggests some form of "carry-over" of sensation. The results for the true movements were tested for this effect without success. It is interesting that subject T, who was so successful in the detection of true movements and in the identification of their direction, should give such a poor display in detecting that no movement was taking place and should be so heavily biased in attributing one particular direction to these.

A comparison of the results with the "nil movements" with those obtained with the true movements can only lead one to conclude that any false sensation of movement has not had any profound influence on the general trends shown by the true movements.

True movements

The significance of the greater sensibility to extension than to flexion movements is interesting. Goldscheider (1889) found the reverse effect in the case of the elbow joint. Laidlaw and Hamilton (1937*b*) noted lower threshold values in general with extension movements than with flexion, but did not find either direction favoured in the case of the elbow joint. Winter (1912), on the other hand, recorded lower threshold values with the extension movements than with the flexion, with a reversal of this effect at some of the higher speeds. However, his results for the two directions are not strictly comparable, as the initial angle from which the movements of the joint started was different in the two cases. It may be important that Laidlaw and Hamilton (1937*b*) noted the preference of extension over flexion in all the joints which they moved vertically. They detected no preference for either direction in the elbow joint, which was the only joint to be moved in a horizontal plane. It was noted by us, in preliminary trials before the present investigation, that the flexion/extension difference was most marked when the elbow was at 10° from full extension, less marked at 30°, and still less at 45°. It is tempting to speculate that the pull of the force of gravity on the two aspects of the limb modified the sensation in some way when passive movement takes place in the vertical plane.

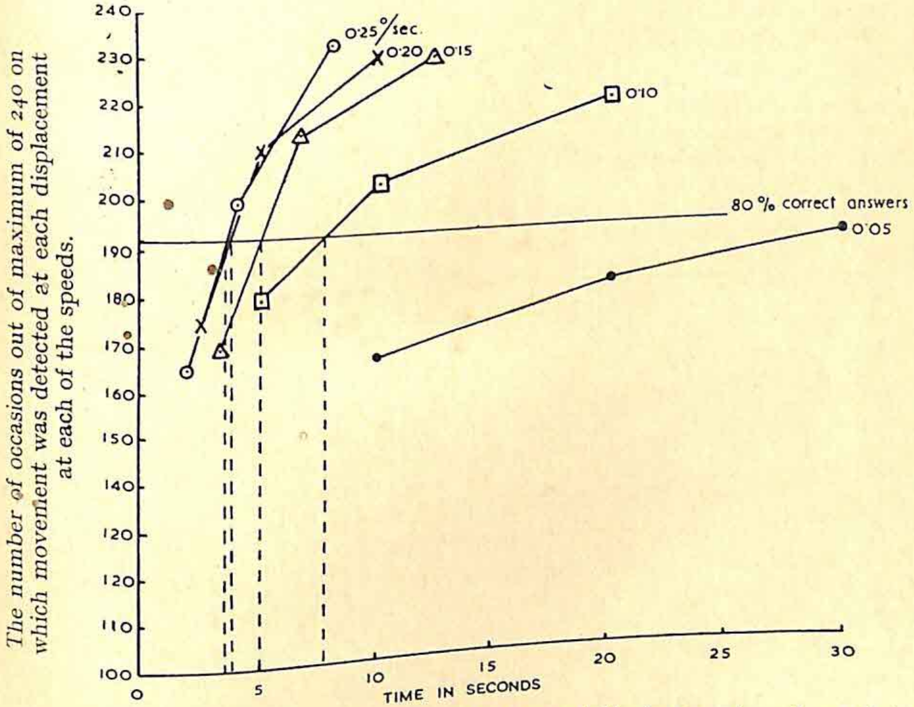

FIG. 1.—The detection of movement at the different displacements and speeds in relation to time. The interrupted vertical lines indicate the estimated duration of the movement necessary for 80% success at each of the speeds.

It was to be expected that the frequency of detection of movement should increase as the magnitude of the displacement increases—the more movement occurring, the more likely it was to be detected. Similarly, one might have expected that the larger the movement is, the less likely would be a subject to mistake its direction.

The increasing perception of movement with increasing speed until a peak level of performance is reached, with subsequent deterioration at the higher speeds, may be explained by the diminishing time factor as the speed increases. At the higher speeds, the movement becomes more of a jolt which may easily be missed with the slightest relaxation of attention. Such an explanation would accord with attainment of the peak performance at a lower speed in the identification of direction, less and less time being allowed for judgment of direction to be made as the speed increased. It must be recorded, however, that the results obtained at the different speeds, particularly in regard to the identification of direction, are in no way clear cut and in the latter case the differences noted are not significant statistically. However, the fact that the speed/displacement interaction was significant also suggests that the time factor is important. In other words, at any particular speed, a certain amount of movement must occur for perception to be reached, more for the identification of its direction than for its mere detection. This is as near to the definition of threshold values as we have reached in the present study.

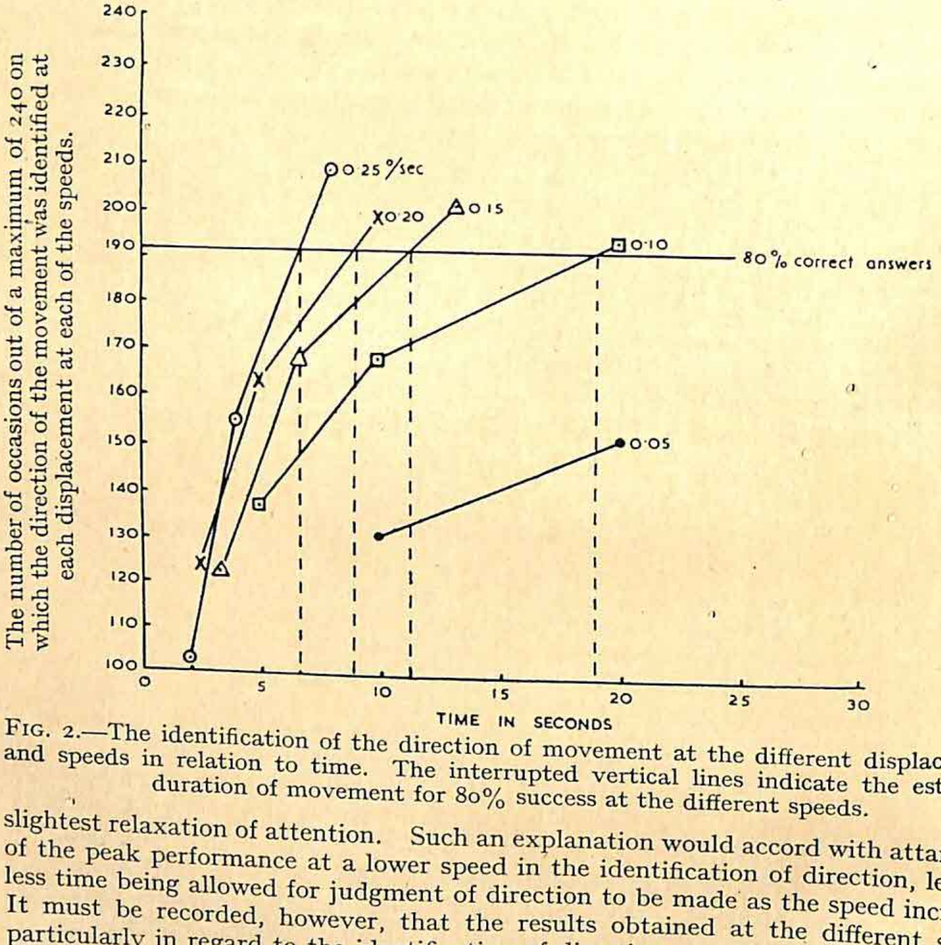

FIG. 2.—The identification of the direction of movement at the different displacements and speeds in relation to time. The interrupted vertical lines indicate the estimated duration of movement for 80% success at the different speeds.

If the figures contained in Table IV are considered from the point of view of duration of movement at the different speeds, certain interesting points emerge. From the graph constructed from these figures, it can be seen that the slope of the curve of performance plotted against time is flattest with the least speed and becomes more and more vertical as the speed is increased (Fig. 1). Selecting 80 per cent.

success as an arbitrary threshold level of detection of movement, it can also be seen that this is achieved with all five speeds at displacements of a similar order, i.e. 0·8°. Although it would be very hazardous to generalize from data as incomplete as this, it may be suggested very tentatively that, with the range of speeds studied, 0·8° represents the minimum amount of passive movement that must occur if 80 per cent. success in detection of movement is to be attained. If the data obtained in the identification of direction are tested in a similar manner, a displacement of 1·8° appears necessary at the four high speeds to reach the 80 per cent. level of performance (Fig. 2). No figure can be obtained for the lowest speed. The close proximity and similarity of the curves at the shorter durations of movement suggest that "jolt" perception is predominating and the separation at the longer durations may indicate a change of sensation to one of true movement. This point is clearly worthy of pursuit by specifically designed experiments.

The sensation experienced

Pillsbury (1901) and Winter (1912) both described the waxing and waning of sensation when the elbow joint was moved passively and also its reference to a point distal to the joint. It is not felt that the present study has contributed anything to the elucidation of these findings. A possible explanation may be that the subjects were unable to attain and maintain complete and continuous muscular relaxation, and the intermittent nature of the sensation may be attributed to the fluctuating activity of associated muscles. This may also account for the sensation experienced during the course of "nil movements." Perhaps evidence for or against this hypothesis could be obtained by examining the muscles electromyographically during passive movements.

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SOME EFFECTS OF REWARD ON THE STRENGTH OF POSITION STEREOTYPES IN THE WHITE RAT

BY

F. KNÖPFELMACHER

(From the Animal Psychology Laboratory, Institute of Psychiatry, Maudsley Hospital, and University College, London)

Certain aspects of Maier's hypothesis that rigid behaviour stereotypes elicited in an insoluble problem situation are not explicable in terms of goal-motivated learning were tested thus: Forty-one white rats of Wistar stock were exposed to an insoluble problem in a water discrimination unit. Each of the twenty-four animals who formed position stereotypes was assigned to one of four groups. One of these groups served as a control and received no special treatment. Each of the remaining three experimental groups was given a different number of successively rewarded trials to the side of the stereotype. Finally, all groups were presented with a soluble problem, and the strengths of the stereotypes in each group observed in terms of the breaking of the stereotypes.

It was found that the strength of the stereotype behaviour was directly proportional to the number of rewarded trials. None of the stereotypes was sufficiently rigid to meet Maier's criterion of "fixated" response patterns. More stereotypes were formed by males than females. On the other hand no sex differences appeared in the subsequent behaviour of animals who did form stereotypes.

These results are interpreted as showing that stereotyped responses formed under conditions of the present experiment are not qualitatively different from learned responses.

I

INTRODUCTION

The experiment reported in this paper was designed to test some of the properties of behaviour stereotypes elicited in an insoluble problem situation. N. R. F. Maier (e.g. 1949); claims that these behaviour stereotypes, which he calls behaviour "fixations" when they fail to break under ordinary extinction procedures, differ qualitatively from behaviour patterns developed under conditions of goal-motivated learning. Before Maier's hypothesis can be accepted or rejected it seems necessary to submit the behaviour manifestations in question to operations which have been or could be used to test hypotheses of goal-motivated learning. If the results of such operations differ from anticipations based on learning theory, one of the following ways of systematizing the facts is possible:—Either a new explanatory system exclusive of learning concepts could be constructed; or some new concepts could be defined and incorporated into suitable explanatory systems already in use; or a skilful application of an established unamended conceptual system might explain the results.

It seems important to experiment with different kinds of environmental conditions for several reasons in addition to that of avoiding misplaced generalizations. Different experimental results obtained under these different conditions may be co-ordinated with suitably analysed differences of environmental conditions. Explanatory concepts may then be suggested accounting for the differences of results as related to the differences of environmental conditions. Such concepts may form the basis of a new theoretical system or may help to extend or amend an older theoretical system.

An experiment performed by Russell and Pretty (1951) was a step in the direction suggested. Rats set an insoluble problem in a water discrimination unit formed behaviour stereotypes significantly more rigid under certain extinction procedures than the habits formed in a soluble problem situation by a control group. However, these stereotypes differed from those formed in Maier's experiment mainly in that they all were extinguished by concentrated punishment. Explanations suggested by Russell and Pretty account for the differences between their experimental and control groups in a manner compatible with learning theory, but do not account for the difference between their experimental group and Maier's fixated animals.

A further study of the nature of such stereotypes in experiments relevant to learning theory is to be reported below. Following are some of the considerations which have led to the design of the experiment:—Maier assumes that rigid behaviour stereotypes are "frustration-instigated" and that frustration accumulates as the behaviour stereotypes are undergoing punishment through the experimenter's attempts to break them. If the rigidity of the responses is a function of frustration, increased punishment should not only fail to decrease their rigidity but actually increase it. It seems to follow from this that if the frustration increase were halted, the stereotype should, at most, become more amenable to breaking and, at least, not less amenable.

If an organism exhibiting a behaviour stereotype elicited by frustration is placed in an environment in which the behaviour is followed by reward, one would certainly expect the cumulation of frustration to stop and one could perhaps expect a decrease in the effect of previous frustration. Thus reward ought to have effects on behaviour stereotypes contrary to expectations from the learning principle that the reward of a habit increases its strength provided the physiological limit has not been reached. If the strength of a position stereotype is not increased by reward, or if it is decreased, explanations in terms of learning theory would be rendered less plausible. The converse result would support the application of learning principles.

The present experiment was designed to test the effects of reward on the position stereotypes in the sense outlined above, firstly, by observing the relation between the amount of reward following the establishment of the stereotypes and the ease with which they are broken and, secondly, by observing the number of trials between the points at which a stereotype was broken and the point at which the new habit was learned. These trials will be referred to as "breaking-learning" or "B-L" spans.

The B-L spans seem to be important as indices of latent learning. Maier claims repeatedly that fixated rats show less resistance to responding when the positive symbol is on the side of the stereotype. When the negative symbol is on this side, the animals show a resistance to responding, which can be measured roughly in terms of the intensity of stimulation—electric shock or air blast—required to produce a response. Qualitative differences in the method of responding also are observable when the negative as opposed to the positive symbol is on the side of the stereotype. These observations seem to indicate that latent learning is taking place to a much higher degree in "fixated" than in "non-fixated" animals. The latter abandon a habit without difficulty under ordinary extinction procedures. It follows that if a method were found of alleviating or preventing rigid behaviour-stereotypes, the probability of latent learning taking place should decrease as its efficacy increased. If, therefore, reward were effective in alleviating the rigidity of behaviour stereotypes, one would expect to find that the more the reward, the less the latent learning and consequently the longer the B-L spans. If, on the other hand, reward strengthens the stereotypes, as one would predict from the learning principle, no difference in B-L spans would be expected.

The present research design differs from previous studies in that the symbols upon which the soluble problem was based were introduced only during that phase of the experiment and were not present during other phases.

II

METHOD

The procedure is outlined in Table I. All animals were treated identically except for phases 3 and 5. The independent variable—number of successively rewarded trials following development of the stereotype—was varied in phase 3. Only those animals who failed to break their stereotyped responses under conditions of 50 per cent. punishment entered phase 5.

TABLE I

RESEARCH DESIGN

Group	Phase				
	1	2	3	4	5
I	Preliminary training	Insoluble problem	60 Trials rewarded	Soluble problem 50% punishment	Soluble problem 100% punishment
II	,,	,,	120 Trials rewarded	,,	,,
III	,,	,,	180 Trials rewarded	,,	,,
IV	,,	,,	0 Trials rewarded	,,	,,

Apparatus. The water discrimination unit described by Russell and Pretty was used. This apparatus was altered in only one respect, the length of the partition dividing the doors being increased from 6 in. to 9 in. to eliminate all possibility of animals hanging with their hind legs on the partition and trying to open the door with their forelegs.

Subjects and technique of grouping. Forty-one white albino rats of a highly inbred Wistar stock, comprising six litters, were used. Their ages varied from eight to nine months at the beginning of the experiment. Animals of both sexes were included, the original sample consisting of 21 females and 20 males. Three experimental and one control group, each containing six rats, were formed from those animals which developed position stereotypes during phase 2 of the research design. The groups were matched on the number of trials to reach the criterion of stereotype. The matching was done for males and females separately, a precaution justified by the results. The subjects lived in a special series of cages in the animal colony room and were maintained on a diet of M.R.C.41 and Thompson rat cubes and water.

Procedure. The water in the discrimination unit was kept constantly at 18 ± 1° C. during all experimental periods. The temperature of the room was kept at 70 ± 1° F. The windows of the room were darkened and the room illuminated by two electric bulbs hanging from the ceiling. The rats were brought singly from the adjacent animal room into the experimental room and placed at the starting point in the discrimination unit. After each trial, records were made of the door to which the animal first swam and whether the door was locked or unlocked. As soon as the animal chose the unlocked door and escaped from the water, the animal was put back into the water in the same way as before. A daily session consisted of ten such trials given in succession to each rat throughout all the stages of the experiment subsequent to preliminary training. At the end of the ten trials the rat was dried and returned to its home cage.

The symbols were covered in phases 1 and 2 of the experiment, but were uncovered and manipulated in a manner to be described in all the other phases. This was done to exclude the formation of symbol stereotypes since the research design demanded only the use of animals developing position stereotypes.

Phase 1. Preliminary training. All animals were given preliminary training for five days, six trials per day. The first part of the preliminary training lasted for three days during which one door of the apparatus was kept wide open and the other locked. The left and the right door were opened in random order to ensure that no particular patterning of responses was rewarded. During the second part of the preliminary training, lasting for two days, one door was locked as before and the other kept half open by means of a wedge which could be pulled away thus opening the door when the animal attempted to escape through it. A random order of locked and unlocked doors was adhered to as before.

Phase 2. *Insoluble problem.* Both doors were closed and an insoluble problem presented by furnishing no cues as to which of the doors was unlocked. This was accomplished by locking and unlocking the doors in a random sequence, on each trial one door being locked and the other unlocked. Each animal was given ten trials per day, the experimental sessions being spaced at approximately 24 hours intervals. Maier's criterion for the establishment of the stereotype was adhered to as closely as possible. This requires that the animal swims to the same door with not more than five deviations during the last 160 trials. If this criterion was not reached within 500 trials the animal was discarded for the purposes of this report.

Phase 3. *Reward.* As soon as the animal reached the criterion of stereotyped behaviour it was assigned to one of the four groups required by the research design. Experimental groups I, II and III now received various numbers of successively rewarded trials. The control group IV was put directly on phase 4. In the experimental groups the stereotype was rewarded by always leaving unlocked the door on the side of the stereotype. Groups I, II and III received 60, 120, and 180 of these rewarded trials, respectively. The symbols fixed above the doors—a black circle on a white background and a white circle on a black background—were now uncovered. The former was made positive by being attached to the unlocked door.

Phase 4. *Soluble problem,* 50 *per cent. punishment.* In this phase all animals were presented with a soluble problem. The unlocked door was varied from side to side in random order irrespective of the rat's stereotype, but the positive symbol was always above the unlocked door. This was possible since the symbols were attached to the apparatus in a manner which enabled them to be shifted from door to door.

The animals were now required to break their position stereotypes and to learn the correct symbol habit. There were two requirements for the breaking of a stereotype: first, five non-stereotype responses and, second, not more than ten trials to the stereotype side between successive non-stereotyped responses. The first response of such a series of five deviations was considered to be the point at which the stereotype was broken. The learning criterion of tne symbol problem was reached when the animal made seven consecutive responses to the positive symbol in one day's trials.

The breaking criterion is admittedly arbitrary and was derived from inspecting the behaviour of other rats in preliminary experiments. It appeared that a single response to the non-sterotyped side during a series of sometimes more than 400 stereotyped responses could not serve as a satisfactory index of breaking because of the freedom of movement allowed the rat by the nature of the apparatus. It might be objected that the density of the five non-stereotyped responses is arbitrary. The results showed, however, that the animals which made five responses to the non-stereotype side with more than ten stereotype responses in-between, always made a smaller total number of responses to the non-stereotype side than animals whose deviation sequence possessed the required density.

If an animal did not reach the breaking criterion after 300 trials on the 50 per cent. punishment basis it was advanced to phase 5 where it received 100 per cent. punishment for making the stereotyped responses. If an animal broke its stereotyped response during 300 trials in phase 4, but did not reach the learning criterion, training with 50 per cent. punishment was extended for another 300 trials. This was the maximum number of runs which any rat was allowed to make in phase 4. All rats which broke during the first 300 trials on the 50 per cent. basis did in fact learn during the 600 trials. This extension of training during phase 4 made it possible to obtain data on both breaking and learning under the same conditions of punishment.

Phase 5. *Soluble problem,* 100 *per cent. punishment.* If an animal did not reach the breaking criterion after 300 trials on phase 4 it was advanced to phase 5. The door on the side of the stereotype was kept locked with the negative symbol above it, the unlocked door with the positive symbol remaining on the non-stereotype side. Every response to the stereotype side thus met with punishment. This procedure was continued to a maximum of 300 trials to break and 600 trials to learn. 100 per cent. punishment of the stereotype was continued till the animal made five consecutive responses to the non-stereotype side. When that happened it was required to swim twice consecutively to the stereotype side.

previous stereotype side and when this was achieved the doors were randomized again on the 50 per cent. soluble symbol basis. When an animal went five times in succession to one side, i.e. re-established a stereotyped response, 100 per cent. punishment was re-introduced and continued to a point at which the animal made two responses to the side opposite from its new position habit. Randomizing on a 50 per cent. basis was then re-introduced. This continued until the learning criterion was reached. Phase 5 differed as can be seen, from phase 4 by the fact that attempts were made to extinguish the stereotypes and all subsequent position habits by concentrated punishment until the learning criterion was reached.

III

RESULTS

The results are summarized in Table II. Twenty-four of the forty-one animals formed position steroetypes. All animals eventually broke their stereotypes. Those animals who failed to reach the breaking criterion with 50 per cent. punishment reached it with 100 per cent. punishment. All animals learned the symbol problem after breaking their position stereotypes, although their B-L spans differed considerably.

Breaking. The groups differed significantly in their tendency to break under 50 per cent. and 100 per cent. punishment. Figure 1 shows that the number of animals breaking under 50 per cent. is inversely related to the number of rewarded trials in phase 3. The proportion of animals requiring concentrated punishment to reach the breaking criterion increased as reward increased.

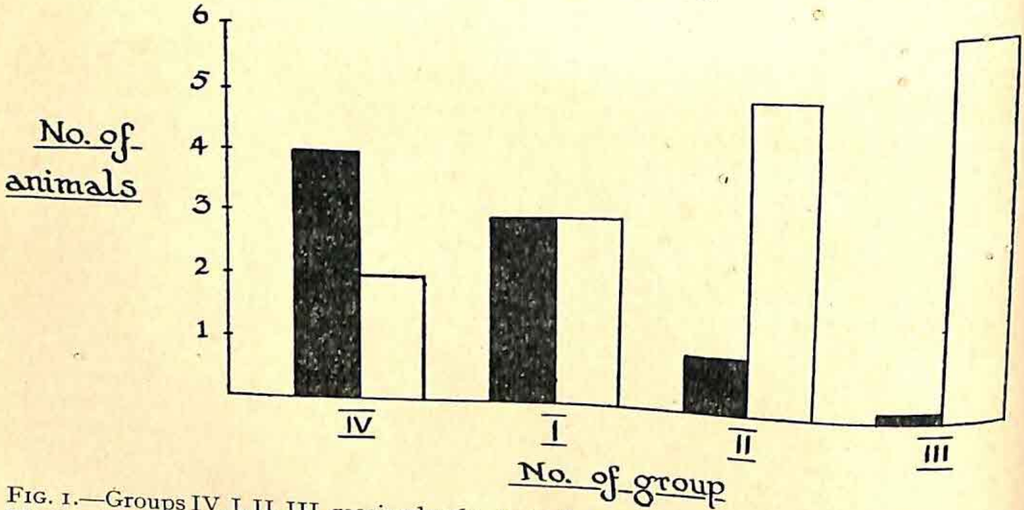

FIG. 1.—Groups IV, I, II, III, received 0, 60, 120, 180, rewarded trials respectively in Phase 3. Black columns—animals reaching breaking criterion under 50% punishment; open columns—100% punishment.

Both the deviation from homogeneity of the population and the trend of this deviation had to be analysed. The trend of the deviation seems by far the more important factor relevant to the hypothesis, as, on the goal-motivational learning hypotheses one would expect the rigidity of the stereotype to be correlated with the amount of reward given. To test the trend, Spearman's rank-correlation formula was applied. The groups were ranked on the number of rewarded trials in phase 3, and on the proportion of animals that had to be given 100 per cent. punishment to reach the breaking criterion. It can be seen from the data that $\rho = 1$. The

TABLE II

RESULTS

No. of column									
1	2	3	4	5	6	7	8	9	10*
				Trials to break		*B-L spans*		*Trials to learn soluble problem*	
Group	*Animal*	*Sex*	*Acquisition trials insol. prob.*	50%	100%	50%	100%	50%	100%
I	1	M	270		83		319		402
	2	M	200		6		126		132
	3	F	240		80		141		221
	4	M	160	239		132		371	
	5	M	210	262		29		291	
	6	F	280	120		412		532	
Mean			226·6	207·0	56·3	191·0	195·3	398·0	251·6
II	7	F	470		49		302		351
	8	M	420		38		274		312
	9	F	280		28		124		152
	10	M	270		4		26		30
	11	M	160		36		43		79
	12	F	210	225		38		263	
Mean			301·6	225·0	31·0	38·0	153·5	263·0	184·8
III	13	M	300		74		418		492
	14	F	350		29		302		331
	15	M	180		45		98		143
	16	M	200		18		143		161
	17	M	180		15		198		213
	18	F	220		47		166		213
Mean			238·3		38·0		220·0		258·8
IV	19	M	490		25		115		140
	20	F	190		42		39		81
	21	M	420	168		134		302	
	22	M	190	223		8		231	
	23	F	360	220		360		580	
	24	M	180	70		330		400	
Mean			305·0	170·2	33·5	208·0	77·0	378·2	110·5

* The numbers in this column were counted from the first trial in phase 5 of the research design.

significance of this correlation is on the 0·0416 level. To test the level on which the null hypothesis can be disproved irrespective of the trend of the deviation from homogeneity Haldane's test for 2 × n tables with expected frequencies of less than 5 was applied (Haldane, 1945). On this test $\xi = 1{\cdot}30805$ which is significant on the 0·097 level. The two probabilities, 0·0416 and 0·097 obtained from the two independent tests were now combined by Fisher's chi-square method for independent tests of significance. The result showed that the aggregate of the two probabilities is significant at the 0·026 level of confidence.

It may therefore be concluded that increased reward of a stereotype under the present experimental conditions makes it less amenable to breaking under mild, 50 per cent. punishment. The more a stereotype is rewarded the more it becomes probable that concentrated punishment will be required to break it. Reward, therefore, does not *decrease*, but seems to *increase* the rigidy of position stereotypes.

The next step involved determination of whether or not the average number of trials to break within phases 4 and 5 differed significantly between the experimental and control groups. A *t*-test applied to the two extreme mean values of column 5 in Table II gives a *t*-value of less than 1, i.e. the difference is not significant. A similar test applied to the two extreme mean values of column 6, Table II, shows that this difference also is not significant, "*t*" again being less than 1. Though the probability of a stereotype breaking under 50 per cent. punishment significantly decreased with increased reward, our population sample did not yield data pointing to a relationship between the amount of previous reward and the point at which a stereotype reached the breaking criterion within the 50 per cent. or the 100 per cent. punishment phase.

Breaking-learning spans. Columns 7 and 8 of Table II show the breaking-learning spans. A *t*-test between the extreme mean values of column 7 gives a *t*-value of less than 1 and a similar test between the extreme mean values of column 8 gives "*t*" less than 2. Neither of the differences is significant. Hence there is no detectable relationship between reward in phase 3 and the length of breaking-learning spans. This conclusion can be held only very tentatively since the number of animals whose mean B-L spans were compared was very small. However, the unmistakable trend of breaking-reward relationship which counterindicates assumptions of special frustration mechanisms for stereotypes, renders expectations of differences in B-L spans baseless, and supports independently the results of the otherwise not very reliable comparison of mean B-L spans.

Learning. The learning scores are shown in columns 9 and 10 of Table II. A *t*-test between the extreme mean-scores in column 9 gives a *t*-value of less than 1. The *t*-value is less than 2 for the extreme mean scores of column 10. Reward, therefore, does not seem to affect the speed with which a new habit is learned when a soluble problem is introduced. The learning scores are, of course, directly influenced by the circumstances, determined partly by the research design under which an animal can break the position stereotype. If it breaks in phase 4 it will receive its learning trials under 50 per cent. punishment and if it has to be advanced to phase 5 it will receive its learning trials under 100 per cent. punishment. The design of the experiment makes it impossible to compare the learning scores of phase 4 with those of phase 5.

Sex differences. The proportion of males forming stereotypes was significantly greater than that of females. Out of 20 males 15 reached the criterion of position stereotype, whereas out of 21 females only 9 did so (chi-square = 4·36055—significant at the 0·037 level of confidence). No sex differences were found between those males and females which were used in the experimental and control groups. Tests were made of differences in all experimental variables, i.e. onset of stereotypes, breaking, B-L spans and learning, no significant relationship being discovered.

Relation between the number of trials before stereotype formation and the strength of stereotype. In Table III the number of trials to reach the criterion of stereotype is contrasted with the number of trials to reach the breaking criterion. Inspection of a scattergram on which these variables were plotted showed no relationship of any importance.

No statistically-significant differences were found between the four groups in the mean number of trials to form stereotypes (column 4 of Table II).

TABLE III

No. of group	No. of animal	Stereotype acquisition trials	Trials to break	
			50%	100%
I	6	280	120	
	1	270		83
	3	240		80
	5	210	262	
	2	200		6
	4	160	239	
II	7	470		49
	8	420		38
	9	280		28
	10	270		4
	12	210	225	
	11	160		36
III	14	350		29
	13	300		74
	18	220		47
	16	200		18
	15	180		45
	17	180		15
IV	19	490		25
	21	420	168	
	23	360	220	
	22	190	223	
	20	190		42
	24	180	70	

IV

SUMMARY AND DISCUSSION

A chief characteristic of most theories of "goal-motivated" learning is that habits are held to be strengthened by reward and weakened by punishment. When reward and punishment are followed by effects incompatible with those expected from learning theories, inadequacies in these theories may exist. Maier claims that precisely such inadequacies make it advisable to exclude behaviour-fixations from the class of phenomena described and explained in learning theories and to use other concepts for their explanation. His observations indicate that punishment-generated frustration increases the rigidity of behaviour stereotypes to infinity, i.e. "fixates" them. It seems that Maier's theoretical model for "frustration-instigated" behaviour is almost a mirror image of learning models in that punishment increases habit strength and reward or lesser amounts of punishment, counteracting, as it were, the cumulation of frustration and the resulting rigidity of habit, ought to be expected to decrease or at least not to increase habit strength. The rigid, "fixated," stereotype is not motivated by drive-tension decrement or any other state conducive to adaptation and survival. From some of Maier's remarks it may be concluded that he considers the rigid behaviour-stereotype to be in some sense survival- or adaptation-neutral.

Maier's hypotheses were not confirmed by the properties of the stereotypes elicited in the water discrimination unit under the conditions of the present experiment and explanations of the results in terms of "learning" concepts are strongly indicated. The following facts yielded by the experiment stand out clearly.

1. Reward significantly increases the strength of the stereotype, a result to be expected on the basis of goal-motivated learning theory, but hardly to be expected on Maier's frustration theory.

2. Punishment invariably breaks the position stereotype—concentrated punishment breaks it more effectively than diffuse 50 per cent. punishment. None of the stereotypes was sufficiently rigid to meet the criterion of a fixated response pattern. On Maier's theory concentrated punishment, leading to greater cumulation of frustration, ought to make the response less amenable to breaking and increase its rigidity.

The results of our experiment do not warrant a rejection of Maier's hypotheses nor do they prove that all behaviour stereotypes are not incompatible with learning theories. The divergences of Maier's results from expectations based on learning theories remain a fact in spite of the impossibility of getting similar divergences in a different kind of experimental situation. The thing to do next seems to be an analysis of the differences between the apparatus used by Maier and the one used by ourselves. It may well be that the behaviour stereotypes obtained in the water discrimination unit can be explained by learning theories, whereas the stereotypes obtained by Maier on the Lashley jumping stand cannot be thus explained. An analysis of the two kinds of apparatus may suggest an explanatory concept, co-ordinating the different results obtained by the two kinds of operations. The explanatory power of any such concept will have to be tested independently in experiments relevant to the concept. Three differences between Maier's experimental set-up and that used in the present research are apparent: differences in kinds of motivation; different degrees of unavoidable punishment inherent in the situation; and different amounts of kinaesthetic-locomotor response segments which the "correct" and "incorrect" responses have in common on the Lashley stand and in the water discrimination unit respectively.

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I wish to express my gratitude to Professor R. W. Russell, under whose supervision this study was carried out, for his help and advice. I am indebted to Professor G. C. Drew who has read and discussed the first draft of the manuscript in great detail. My thanks are due to Mr. R. A. Jonckheere for his assistance in statistical matters.

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BOOK REVIEW

Thinking: An Introduction to its Experimental Psychology. By GEORGE HUMPHREY. Methuen's Manuals of Modern Psychology. London, 1951. Pp. ix + 331. 21s.

In these days of rapid and often ill-considered publication it is an immense intellectual delight to meet this book, which must be read slowly and carefully and thoughtfully, as it was prepared and written. Nearly 20 years went in the writing, and a great many more in the studies which made the writing possible. Here are a distinguished, rather leisurely, love of exact scholarship; a scrupulous regard for the nature and value of evidence; a determination to leave out nothing essential in description and exposition and to put in nothing inessential; a very honest and fair-minded use of criticism, and over and through everything else the constant exercise of a wisdom and a judgment which many people may be tempted to think are much more rare in current psychological writing and thinking than once they were. If, in spite of all this, the volume leaves other readers, as it does me, with a sense of disappointment in the end, such regrets as may be entertained cannot seriously diminish the lasting importance of an achievement which few, if any, other living psychologists could rival.

The volume begins with a most admirable brief survey of the rise and alleged fall of associationism as a basic psychological theory. It is interesting that Humphrey, who is at least as fully aware of the difficulties of this approach as any other writers have been, is also able to avoid that extremity of enthusiasm which claims that associationism is or, indeed, is ever likely to be, completely demolished.

Without further ado the book proceeds at once to what may be called its main theme, a critical exposition and evaluation of the methods and claims of the Würzburg experimenters who considered that they were dealing directly with the psychology of thinking. The next three chapters, all specifically discussing this work are, to my mind, by a long way the most impressive and successful parts of the whole book.

It has always seemed to me that the course of history treated the Würzburg Group rather roughly. They began a bit before the opening of the 20th century and went vigorously on for nine years or so. In so far as there was any immediate reaction in Germany it was mainly and violently antagonistic, for, as Humphrey shows with great force over and over again, the issues they reached, or approached, were even more at variance with the general associationistic framework within which they still considered themselves to be working, than they fully realized. Still, by the time war clouds were gathering, it looked as if other psychologists, especially in America, were beginning to take this work really seriously and to explore for themselves its methods and claims. But the war came, and after it the temper of the times seemed to be changed, and the balance of emphasis in psychological research shifted decisively away from Germany. The potentialities of the movement initiated by Külpe's amazingly efficient little group seemed to be arrested. Titchener made things no better for them because he set their work into the frame of a type of introspectionism that was already in decline. The Würzburg experiments dropped into an entirely undeserved neglect.

It was partly their own fault. Anybody who has wrestled with their ponderous and profuse experimental reports and theoretical expositions; with their often over-formalized categorizations of ill-defined and poorly articulated experiences; with their horribly difficult terminology and perhaps particularly—with the exception of Bühler, and he probably suffered because of the exception—with their deep lack of humour, must have been tempted over and over again to give up in exasperation and despair. Most of all these are experiments which cannot possibly be appreciated or fairly criticized by people who read about them, but refuse to do them.

The time was ripe for a thorough re-examination of the Würzburg work, and a really informed attempt, not made merely from the outside point of view of the psychological theorist, to set it in its proper place in the history of developing research. These are what Humphrey has done with conspicuous success in his three chapters on *Imageless Thought*, on *The Mechanism of Thinking*, and on outstanding points of criticism. His exposition, based directly on the originals, is much more lucid than they are, although this does not mean that it is easy reading. So far as my own knowledge goes I would assert without hesitation that it is exact, discriminating, generous and fair, alive both to faults and to virtues. I believe that he allows us to see the Würzburg enterprize very much as the men who tried it on saw it themselves.

Is this enough? It may be that for a complete appreciation of any historically important movement it must be viewed, not only as those saw it who shaped it, but also in the light of subsequent movements of which its originators can have had no knowledge at all. Take one point only. Humphrey is greatly worried because, for all their complicated paraphernalia of task, determining tendencies, conscious attitudes, and unique awarenesses, these experimenters never seem to him to have provided what he calls a "motor" for thinking. He comes back to this again and again, even at the end of the book when he is asking what yet remains to be done.

Not only by their method, but also by all the prepossessions of their psychological education, this group was bound to talk of everything they found as if it was a form of awareness. There is a tremendous and long-standing prejudice in favour of regarding any awareness as a mechanism awaiting activation from outside, preferably from emotion or something of the sort. But we have only to do the Würzburg experiments, from the beginning, so to speak, to discover that their "awarenesses" are not really like that at all, however much their language may make them seem to be so. Whenever we come across them they are already loaded with items of material which their activity has assembled and can push about and change in various ways. They are not independent of items of material (or content) except in the sense that their character as processes is not determined, or is only partly determined, by the particularity of the items. As Nuttin has shown experimentally, an awareness can be, perhaps always is, already a "motor."

Take the Bewusstseinslagen. It always seems to me a terrible pity that this word ever was translated into English. If we do the experiments we shall find that, as we appear to be establishing the connexions required for the accomplishment of the tasks they set, there are certain moments when a strong impression ("knowing") arises that there is something tremendously important either in what is just past, or in what is now going on, and that what it can be may be found, as we say, "just round the corner." It is this implication of critical *site*, or *place*, or even *moment* having a position in a serial process, that tends to get lost when we speak of "attitude" with its suggestion of something static confronting something else equally static. Yet when the original experimenters assigned specific names to their Bewusstseinslagen they were terms like "surprise," "astonishment," "suspicion," "doubt," "indifference," "anticipation," "expectancy" and the like. We would perhaps speak of "mounting excitement," or "fading effort," filling up these expressions by reference to the material that is being dealt with and have much the same thing in mind. Maybe what the Würzburg psychologists really meant was not quite what they themselves would have said that they meant.

Whether this way of attempting to assess any historical movement is really justifiable is, of course, matter for argument. It is not the way that Humphrey, no doubt deliberately, chose to adopt, and so what I have tried to say on this point is not a criticism of what he himself has said, but only a wish that he had tried to say more.

After these four chapters the book loses a little of its light. It remains still on a high level of exact description and criticism, but a little of the "go" and *verve* are lost. There is a rather slight chapter on the work of Otto Selz. This may, however, be good enough, for Selz really added little or nothing to Würzburg and Meinong except some over-formalized representations of the diagrammatic kind. He did have a few interesting ideas: that to think is to fill up a gap, that it often requires some process of "schematic anticipation," and that anybody who is in earnest about its nature and conditions must be prepared to look into the ways in which scientific discovery proceeds. Professor Humphrey certainly brings out all of these, but treats them somewhat sketchily.

Following is a discussion of "the Gestalt theory of thought," very much selected and condensed and on the whole leaving the issues undecided.

The next three chapters deal with "thought and motor reaction," "language and thought" and "generalization." While no other writer, so far as I know, has, in one publication, covered the large variety and range of experimental work in these fields anything like as completely, accurately and judiciously as Humphrey has done, the work here assumes much more than before the character of a catalogue with comments. Perhaps the width of the plan made this inevitable, and these chapters certainly provide mines of information for the student who wants more knowledge about what experimentalists have done; but there is not very much to help anybody who is looking for ways of getting beyond whatever has been done already.

So we come to the "summary and conclusions," and also to the principal disappointment. Misgivings set in with the very first sentence: "Fifty years' experiment on the psychology of thinking or reasoning have not brought us very far, but they have at least shown the kind of road which must be traversed." Have they? Three pages later Professor Humphrey attempts his first and last definition of "thinking." "Thinking," he says, "may be provisionally defined as what occurs in experience when an organism, human or animal, meets, recognises and solves a problem." Apart from the rather arbitrary implication that there is, psychologically speaking, no thinking without solution, which would certainly not have been accepted by all of Humphrey's authorities, this definition is exactly in line with what nearly all the people whose work he has discussed would have said. But it is quite hopeless, for in this wide sense of "problem" nothing ever happens at all on a psychological level which is not an attempt to deal with a problem. Put more specifically it amounts to this: that when any organism is confronted by evidence which it treats as incomplete, and tries to make the evidence more complete by using sources of information which are not there in the immediate environment, that organism thinks. No doubt this accords very well with the wide and loose use of the verb "to think," in everyday conversation, but it cannot possibly be employed to direct controlled scientific investigation. Professor Humphrey knows this perfectly well, for when he is discussing the need of a "motor" for thinking he says (p. 310), "It seems likely that both problems and their solutions will ultimately be found to belong not to the psychology of thinking alone, but to the broad field of Psychology itself." If anybody proposed to experiment on the broad field of Physics, or Physiology, he would get very short shrift, and it surely ought to be the same in the case of Psychology.

Consider the variety of the methods that are revealed by Humphrey's discussion. They may demand the perception or recognition of items that are within the environment, but are not normally regarded as parts of the critical situation; they may require remembering in a general sense as in all the varieties of association methods; they may ask for "an image," or imaging, or imagining as in the Jacobson experiments admirably described in this book; they may cover all those and other activities as well as in Bühler's commentaries on apothegms and philosophic *obiter dicta*, or in Duncker's X-ray problem. In truth if we start this way we shall soon find ourselves in the position which Alfred, Lord Tennyson said he got into when he tried to read Ben Jonson: "I can't read Ben Jonson," he wrote, "especially his comedies. To me he appears to move in a wide sea of glue." At any rate we shall soon find ourselves up against a vast variety of very sticky problems.

A survey of Humphrey's kind ought to convince everybody that there is no hope whatever of a genuinely experimental approach to thinking, unless an attempt is made to mark off the ways in which thinking operates on incomplete evidence from the ways in which other mental processes do the same thing. It may be that all we can get is a kind of gentleman's agreement, but even so psychologists have got to be gentlemen enough to arrive at one.

Another thing: if we merely accept the methods of our forerunners we shall almost inevitably be over-preoccupied by what happens at the two ends of the process, by the Task, and by the Accomplishment of the Task, and we shall very likely get terribly tied up in this worry about a motor for thinking. For hardly any of the methods make it necessary to set out in their precise order the steps that intervene between the beginning of the operation and its accepted end, and a good many of them make it impossible to do this. Yet obviously, it is in the sequence and order of these steps that the thinking is actually carried out. In fact one of the great claims of the Würzburg group came to be that in any precise and effective method the steps must be reconstructed after the event. We are supposed to be able to discover the nature and mechanism of thinking best by looking at them when they are already complete. There are some honourable exceptions: perhaps two in particular. The first is the Gestalt approach where—especially by Wertheimer, only his work is not given much attention in this book—an attempt really is made to set up the steps between the propounding of the problem and its alleged "solution." But the problems are mostly very complicated ones, giving no clear order in sequence of the steps, and anyway the work was done when the Gestalt theories had already hardened into principles to be accepted, so that quite a lot of that also was looking back at a thinking process which had already been completed. The other was the large mass of experiments on associated bodily activities, where a real attempt was made to record the steps while they were being taken. But the trouble then is that only one class of steps is selected for recording, and Humphrey, for example, has no difficulty in showing that much is missed out. We ought to find a way in which, operating upon incomplete evidence, in a graded series more and more steps are needed to reach an alleged termination. We should seek cases in which it is possible to know something definite about the number of degrees of freedom open at each move, and be able to show, perhaps, how these become less as the thinking proceeds until, it may be, every subsequent step up to the terminus, and also the order of the moves when there are more than one, become psychologically "the same for all." This is certainly not an impossible demand, and when it is met we may be able to demonstrate how thinking, as one of the ways of operating upon incomplete evidence, differs from other ways. But it means a new approach, a revolutionary change of method, and the complete rejection of the view that we can ever take the everyday conversational use of "thinking" as a basis for its scientific study. Equally it means a revolt against the conventional ways of approaching the long-standing problems of abstraction and generalization, and it means a complete break between epistemological analysis and psychological experimentation.

The reader who studies this book from start to finish will learn an immense amount about what psychologists have done and achieved in their investigations of thought; he is likely to know no more about thinking at the end than he did at the beginning.

But it would be ungenerous to finish on such a note. Professor Humphrey has done what he set out to do marvellously well, so well that it may be our lively hope that some day he will write an even better sequel.

F. C. B.

The Perception of the Visual World. By J. J. Gibson. This book, which was reviewed in Vol. III, No. 3, is now published in the U.K. by George Allen & Unwin Ltd. at 35s. net.

Some Principles of Construction of Group Intelligence Tests for Adults. By Torsten Husén and Sven-Eric Henricson. Stockholm: Almqvist & Wiksell, 1951. Pp. 100. 5 Sw. Cr.

When the first large scale trial of mental tests was undertaken in World War I, there was already considerable experience, some of it experimental and some clinical, awaiting wider application. There was also a theory of what mental tests were for, which, if modest by later standards, was fully adequate to the practice of the time. At the beginning of World War II there was no comparable reserve of unapplied knowledge; the theoretical structure had scarcely altered in essentials since 1914, and there seemed little prospect that psychologists could do more than demonstrate once again the value of tests in the somewhat crude classification of large populations. The interval between the wars had been mainly devoted to the standardization and refinement of methods, to controversy over technical detail, and to the seemingly endless attempt to keep one step ahead of those who were to take the tests. Moreover, it was becoming evident that mental testing had run into a period of diminishing returns. Each modification in the construction, administration or scoring of tests required a labour of computation and analysis steadily more disproportionate to the gain in predictive power.

In these circumstances tribute must be paid to the work done by service psychologists, continuously under fire for leaving undone what they had never claimed to do or, alternatively, for doing what they had never done and never wished to do. Just how much work was involved in producing and standardizing tests for the Armed Forces has already been made clear by British and American publications. Dr. Husén and Hr. Henricson provide yet another careful record of the results in this field. The Swedish Induction Test (I-Test) was prepared in a new version in 1948. (Induction, it should be recalled, like Intelligence, has a different and more concrete significance in military affairs.) It was designed to supersede the Classification and Induction Test used in 1944–1947, and was based on two years' research during which 406 intercorrelations were computed, each of them by two different methods. Coefficients of reliability and validity (against officers' ratings) were found for each component of the test, and extensive factor analyses were carried out; finally, a new and presumably better test emerged.

Dr. Husén and Hr. Henricson place great emphasis—too much for the reviewer's taste—on the "innards" of the test, its reliability and factorial structure, while they deal summarily with validation. The Spearman-Brown Prophecy formula and Spearman's correction for attenuation make an unwelcome appearance, and the surprising claim is made that "factor analysis liberated test construction, to a certain extent, from depending upon subjective estimates of various test types." Factor analysis, surely, is just as dependent on "subjective estimates" as any other method. Those factors which are identified with the greatest regularity are those which had been distinguished before factorial methods were used. Indeed it is difficult to see how any truly novel factor could be identified: it is usual, if a factor is found which will not fit into any of the traditional categories, to dismiss it as "psychologically meaningless." Apart from a few reservations of this kind, the book is a useful and mercifully direct guide to current practice in test construction. That it contains little of theoretical novelty is the fault of the times, not that of the authors.

A. D. H.

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DISCRIMINATION LEARNING AND TRANSPOSITION BEHAVIOUR OF RATS IN A WATER TANK APPARATUS

BY

IAN M. L. HUNTER

(*Institute of Experimental Psychology, University of Oxford*)

The learning, by albino rats, of a size discrimination in a water-tank apparatus is described. The earliest discriminative behaviour of each of the six successful rats is of the one-look type in which the negative stimulus plays the major role. With further training, four of those subjects develop two-look discrimination in which relational properties of the stimuli are important as shown by the appearance of, first, one-step and, then, two-step transposition. The water-tank and jumping apparatuses are briefly compared. Evidence from studies of rats, monkeys and children is presented for the generalization that, within limits, the effect of practice on discriminative behaviour involving stimulus relata is to strengthen relational responding.

I

INTRODUCTION

In any size-discrimination experiment, the subject is presented with two areas of like shape but different size and trained to react differentially to them, e.g. to approach one and avoid the other. Suppose now that, although the larger area is presented with a still larger and the smaller with a still smaller area, the subject continues to choose whichever is larger (or smaller, depending on the direction of training); transposition of the learned discriminative behaviour is, then, said to have occurred. Discrimination and transposition in rats have been investigated almost exclusively by means of the Lashley Jumping Apparatus: Munn (1950) describes this apparatus and reviews studies employing it. The present paper does not seek to test any hypotheses but merely to describe the learning and transposition of the discriminative behaviour of rats in a water-tank apparatus.

II

EXPERIMENT ONE

1. *Subjects*

The subjects were thirty-two (thirty female and two male) albino rats of the Wistar strain. At the start of the experiment, they were about 60 days old and experimentally naïve, although accustomed to being handled.

2. *Apparatus*

The apparatus consisted of a galvanized iron tank 16 in. wide, 35 in. long and 18 in. deep, in which there were 9 in. of water. The tank's length was divided into two sectors of 7 in. and 28 in. by a sheet-metal inset in which, at water level, were two hinged doors. Each door could be pushed open by the rat to form a bridge from the water in the major sector to the dry platform attached to the end wall of the minor sector and each could be locked from behind independently of the other. Inset, doors and flange were all painted matt black. The areas to be discriminated were white squares on black grounds painted on sheet-metal cards, measuring 5·5 in. wide and 6·0 in. high, such that the base of the square coincided with the middle of the card's lower edge. The cards slid between narrow vertical channels to fall securely into place immediately above the doors and 2·5 in. distant from each other. Nine cards, represented by letters A to H inclusive, were prepared having squares of side 2·1, 2·8, 3·6, 4·7, 6·0, 7·9, 10·3 and 13·3 cm. respectively. The ratio of sides A:B, B:C, etc., was, therefore, 1:1·3 and of areas A:B, B:C, etc., was 1:1·7. Since brightness per unit area was constant, differences of total brightness accompanied differences of size. The temperature of the water was about 18° C.

Throughout, "right" and "left" refer to that side or door lying to the rat's right or left as it approaches up the major sector towards the inset. A "trial" begins when the rat is placed in the water and ends when it escapes to the platform; it is "wrong" if the negative door is touched and "correct" if, regardless of the path pursued, the rat escapes without touching the negative door.

3. *Preliminary Training*

During preliminary training, which lasted for 5 days with ten trials daily, the rats learned to swim the length of the major sector, push open one or other of the doors and escape. On the first two days the doors were open and, on the remainder, they were closed but unlocked. On each trial the rat was picked up and placed in the water at the far end of the tank facing away from the doors so that it had to re-orient towards them: half of the rats were released while the experimenter stood on the right side of the tank and half while he stood on the left. Each rat was given, scattered over the preliminary training period, an average of five additional trials on which it was released facing the doors. Unless otherwise stated, there were about two trials per minute with a minimum intertrial delay of 20 seconds. Between experimental sessions the subjects were kept in their living cages with a plentiful supply of food and water. Records were kept of the time between release and escape as measured by a stop-watch, of the path swum and of the rat's general behaviour.

When placed in the water on the first trial, but never subsequently, each and every rat lay floating motionless with limbs outstretched for some 2 seconds, presumably because of the situation's extreme novelty. Then, came an explosion of activity. The rat twisted, turned, scrabbled violently at the walls as though to scale them, churned and threshed the water, propelled itself jerkily along the walls till one of the open doors was reached and scrambled through the door to begin investigating the platform. The average escape time on trials one to four was 10, 7, 5 and 4 seconds respectively: on the ninth trial the average dropped to 3 seconds, where it stayed for the remainder of the experiment. On the third day, when both doors were closed, the comparatively smooth escape pattern of the first two days was undisturbed in only seven rats, the others pausing or swimming about—sometimes for as long as 40 seconds—before approaching either door. At first, the door was opened either by the rat's crashing headlong into it or by its knocking against it accidentally while struggling to climb the wall above but, by the end of the fourth day, opening was achieved more smoothly by raising the forelimbs during approach and "pushing." As training continued, individual differences became more pronounced but individual performances more self-consistent, the rat re-orienting by always turning to one side and escaping always by a particular door which was opened by a particular movement. Minor variations were ever present, nevertheless, even in an individual performance. When released—and sometimes before, the animal being felt to twist in the hand—the rats re-oriented promptly to face the doors and, in the few trials when they were released already facing them, a number (diminishing from twelve on the second day to three on the fifth) continued to twist round inappropriately in greater or less degree. As a group, the rats had no preference for right or left and the side from which they were released bore no relation to the door chosen.

4. *Discrimination Training*

Discrimination training aimed at the rats' learning to approach that (positive) door above which appeared a certain card and avoid the other (negative) door over which was a certain different card. The position of the positive door was, on successive trials, rlrrllrllrlrllrrlrrl. A response to the unlocked positive door was rewarded by immediate escape from the water while a response to the locked negative door was punished by delaying escape until the rat had circumnavigated the flange to reach the unlocked door. There were two conditions of training under each of which half of the rats were trained on Card E(+) *v.* Card H(−) and half on Card D(+) *v.* Card A(−).

The first training condition allowed twenty-four rats ten trials each per day with the positive door closed and the negative door locked. By trial 150 each rat showed a consistent R- or L-going position habit. Starting on trial 151, the preferred side was made negative for as long as it continued to be chosen and, in this way, sixteen rats lost their position habits in 40 to 100 trials, although eight retained them even after 100 trials. The rats whose R-going habits had been broken now exhibited L-going habits and vice versa. After each rat had had a total of 250 trials, training was discontinued although, as will be seen below, discrimination might have developed later.

The second training condition gave each of eight rats two or three series of ten trials every day with the positive door open and the negative door locked. The minimum inter-series interval was one hour. Position habits quickly developed but, on trials when the preferred side was negative, some of the rats soon began to halt just short of the closed door before taking the detour to the open door. This turning aside was performed at progressively greater distances from the closed door and, by trial 60 to 220, five rats were making consistently errorless "one-look" (*see* below) choices. When, after 40 near-errorless consecutive trials, the positive door was closed so that the cards alone provided differential cues, four rats adopted position habits and only one (a male) continued to discriminate. When each rat had had about 300 trials, training was discontinued with all except the discriminator—hereinafter referred to as "Rat I"—who was trained further and tested for transfer to other pairs of cards.

5. *Rat I's Behaviour Throughout the Learning Series*

After preliminary training, Rat I was given, over nine days and eighteen sessions, a total of 180 trials on discrimination E(+) *v.* H(−) with the positive door open and the negative door locked. On trial 26, the rat halted half an inch from the negative door and turned, without touching it, to swim round to the positive door (Figure 1*a*). This change from approach to avoidance recurred with increasing frequency and at an ever greater distance from the negative door until, by trial 180, the rat would, typically, turn, swim to within about nine inches of the right door and, although only the right door could be visible from this point, either continue or deflect to the left, whichever was appropriate (Figure 1*b*). Such discriminative behaviour may be called "one-look" to contrast it with "two-look" discrimination in which both cards are looked towards before either is approached. With trial 181, differential cues from the doors were removed by having both closed, one locked and the other unlocked. That Rat I had been avoiding the closed door was shown, on each of the next five trials, by its swimming several times to and fro in front of the doors before touching either (Fig. 1*c*). After random success for a few trials, errors became fewer until there was only one between trials 200 and 220. The rat had, perhaps, learned the size discrimination in less than twenty trials or had been responding, all along, to some combination of card and door cues—there is no evidence to decide the point. Beginning with trial 193, Rat I showed two-look discrimination, pausing just in front of the flange and repeatedly moving its head and body in the direction of each door in turn. As training proceeded, these gross, saltatory movements became slighter until they imparted little more than a slight waviness to the smooth, unhalting path down the centre of the tank (Figure 1*d*). There were still, however, many trials on which the centre path was not taken and on which there were changes of direction and of swimming speed. On trial 220, the discrimination was made finer by replacing E by F, but the substitution did not seem to affect performance.

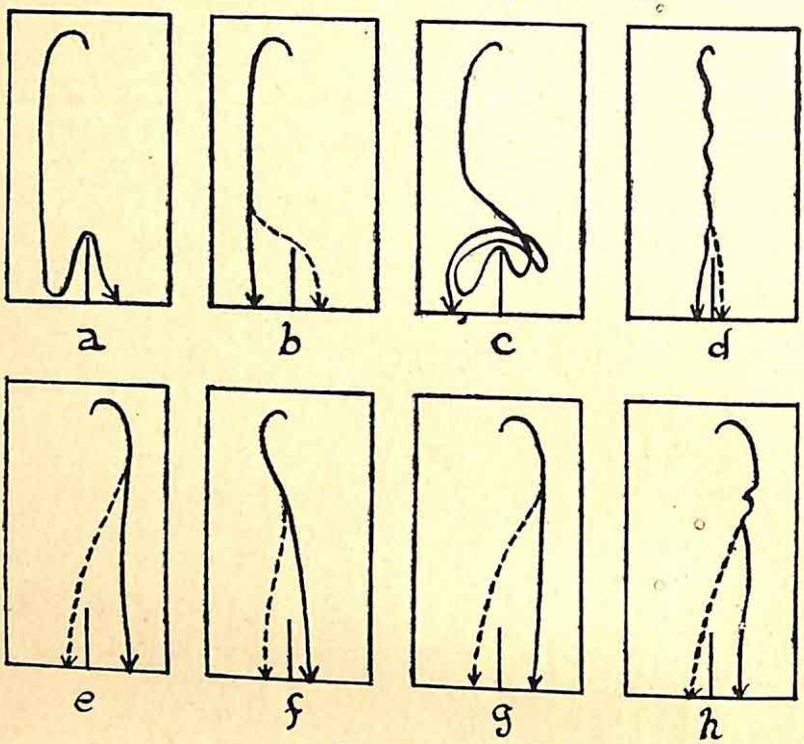

FIGURE 1.

Diagrammatic representation of swimming paths (ground-plan).

Since, between trials 200 and 300, there were only 22 errors, the discrimination was regarded as learned and analysis of the differential cues was begun by observing the rat's behaviour when presented with various new card pairs, i.e. by applying the Method of Equivalent and Non-equivalent Stimuli (Kluver, 1933). In all, there were 125 test trials (with each door closed but unlocked so that choice of either led directly to escape) interspersed among 500 training trials (with positive door closed and negative door locked) and spread over twenty-five sessions requiring six days. There was an average of one trial per minute. Different test pairs were presented in a more or less random order and successive presentations of the same pair, with the relative positions of its members alternated, were spread as widely apart as possible. Now, presumably, the test results would only be completely reliable as indicators of the critical cues if the learned discrimination were perfect—which it was not. A method of increasing test reliability was suggested by the observation that, during training, errors tended to occur in runs of three or four consecutive trials rather than in isolated trials, i.e. a correct trial was more likely to occur among correct than among wrong trials. Accordingly, single test trials were scattered among training trials and the results rejected of any test which was not immediately preceded and followed by a correct training trial. The cues reacted to on the retained test trials would be more likely to be those involved in the learned discrimination than those reacted to on a trial selected at random. Of the 125 tests, the results of 106 were retained and will now be described.

When no cards were present and only black wall appeared above the doors, the rat swam unhesitatingly to the right door. This was in line with the observation that errors on training trials always involved choosing the right and suggested that that door was approached on "indiscriminate" trials when the critical differential cues were not being reacted to. On nineteen tests, when either F or H was presented unopposed by any other card, the no-card side was chosen with but two exceptions. Both exceptional trials—one involving H and one F—resulted from a choice of the right side and were attributed to indiscriminate behaviour. The no-card choosing, while subject to diverse interpretations, revealed that the discriminative behaviour was not describable in terms of "approach-isolated-F" and "avoid-isolated-H"; were this so, the rat, when faced with F *versus* no-card, would, on some half of the trials, have turned first toward F and, without further ado, approached it—this did not happen. Nor was no-card choosing attributable to transposition of "approach-the-smaller area" since, as will be seen below, transposition did not, at least in the early stages,

TABLE I

Trial No.	*One-step*	*Two-step*
369		*n*
377		*n*
427	*t*	
446	*n*	
454	*n*	
537		*n*
586	*n*	
615	*t*	
641	*t*	
648		*t*
660	*t*	
668		*t*
674		*t*
687	*t*	
695		*n*
698		*t*
711		*t*
738	*t*	
742	*t*	
746		*n*
756	*t*	
775		*n*
780		*n*
783	*t*	
797		*n*
806		*t*
817	*t*	
820		*n*
825		*t*
833		*t*
837	*t*	
864		*t*
878	*t*	
884		*t*
889		*t*
893	*t*	
899		*t*

TABLE II

Trial No.	*One-step*	*Two-step*
777	*t*	
789	*n*	
790	*t*	
825	*n*	
835	*n*	
981	*n*	
1031	*t*	
1048	*t*	
1049	*t*	
1051		*t*
1056		*n*
1067	*n*	
1132	*t*	
1136		*t*
1141		*t*
1156	*t*	
1162		*t*
1165		*n*
1186	*t*	
1255		*t*
1258	*t*	
1264		*t*
1265		*n*
1275	*t*	
1277		*t*
1278		*t*
1284		*t*
1319		*t*
1324		*t*
1334		*t*

TABLE III

Trial No.	*One-step*	*Two-step*
524	*n*	
629	*t*	
637		*n*
639	*t*	
693		*n*
698	*t*	
700		*n*
717	*t*	
750	*t*	
759		*t*
797	*t*	
845	*t*	
894	*t*	
955	*t*	
1099	*t*	
1104		*t*
1105		*n*
1174	*t*	
1231	*t*	
1234		*n*
1247	*t*	
1248		*t*
1272		*t*
1305	*t*	
1308		*t*

TABLE IV

Trial No.	*One-step*	*Two-step*
843	*t*	
846	*t*	
852	*n*	
853	*t*	
858	*n*	
863	*t*	
865		*n*
886	*t*	
973	*t*	
975	*t*	
976	*t*	
1054		*t*
1066	*t*	
1098	*t*	
1103		*n*
1106	*t*	
1125		*n*
1126	*t*	
1141	*t*	
1143		*t*
1215	*t*	
1216		*t*
1351		*t*

occur with card pairs which were much more similar to the training pair than were those under present discussion.

There were thirty-two acceptable tests between trials 300 and 554. Fifteen tests involved pairs between the members of which the size ratio was the same as that between the members of the training pair. These pairs were either one-step (i.e. D *versus* F) or two-steps (i.e. B *versus* D) removed from the training pair. The right door was chosen on all but one trial. On eleven trials, when either D, C or A was opposed to H, H was always avoided but, on six trials when either C or A was opposed to F, the right door was chosen. These and the previous tests indicated complete non-equivalence between the training pair and any test pair of which H was not a member and pointed to avoidance of H as the chief feature of the discrimination.

Various test pairs presented on trial 557 and later elicited behaviour different from that described above and revealed, beyond doubt, the presence of transposition. The main results of the transposition tests made throughout the experiment are summarized in Table I. In the first column is the number of the trial on which the test was given and, in the second and third columns, is indicated the occurrence of transposition (*t*) or non-tranposition (*n*) with test pairs, respectively, one-step and two-steps removed from the training pair. The table shows the outcome of only half of the transposition tests made because, as mentioned above, the right side was always chosen on indiscriminate trials so that, when transposition involved choosing the right door, there was no means of telling whether the rat's behavour was transpositional or indiscriminate. Thus, while, to avoid differential reward effects, equal numbers of transposition tests were made in which the smaller area was on the right and in which it was on the left, only the results of the latter are shown. Table I indicates that two-step transposition (not stable till trial 825) developed later than one-step transposition (stable by trial 615).

Over the entire learning series, Rat I's performance showed systematic modifications to which this condensed account does less than justice. Struggling and squealing disappeared, defecation grew rarer and the inter-trial period was spent, not hunched up and shivering in a corner of the platform, as at first, but in fur-licking, scratching and exploration. The escape route became increasingly economic of time and movement by, at one extreme, gradual modifications of established performance patterns and, at the other, sudden appearances of new patterns. An example of gradual change was the moving back of the choice-point, over almost 700 trials, from a locus immediately in front of the right door to one about two feet equidistant from either door. A sudden change occurred on the first discrimination training trial. Before this trial, the rat had learned to swim from the starting point to one or other of the open doors, but had never, at any time, swum between one door and the other. On this trial, the left door was locked, the rat swam to it, bumped against it, struggled and scratched at it, stopped and lay floating with limbs outstretched and the toes of one hind limb gripping the base of the door. Suddenly, the rat detached itself and, with a speed and directness totally unlike "random" activity, rounded the flange to escape *via* the right door.

III

EXPERIMENT TWO

1. *Introduction*

The previous experiment established trends in the discriminative behaviour of a single rat, but gave no information about the generality of their occurrence. Do other rats, in the same situation, show those trends? Do animals of other species, in comparable situations, show those trends? The present experiment attempts to answer the first question; the second question will be discussed later.

2. *Subjects and Apparatus*

The subjects were seven of the female albino rats which had taken part in Experiment One. They had learned no new discriminations in the interval and, at the start of the experiment, were about 46 weeks old.

The apparatus was as previously described with additional illumination of the cards by a 75-watt lamp shining from above the starting end of the tank.

3. *Training Procedure*

Each rat was given, on the first day, twelve trials with both doors open and, on the second day, twelve trials with both doors closed but unlocked. On the third day training was begun on discrimination No-card(+) *v.* H(−). Two training sessions were given daily, usually one in the morning and one in the evening, with twenty trials in each session for the first 10 days and thirty in each thereafter. When a rat had reached a criterion of fifteen correct out of twenty consecutive trials with No-card(+) *v.* H(−), it was given training on F(+) *v.* H(−). The general procedure differed from that in Experiment One in only two respects. Firstly, from the start, trials were given at about one per minute. Secondly, right- and left-going position habits were not allowed to become established for, after one door had been chosen for four consecutive trials, it was made negative until the other door had been chosen twice in succession.

4. *Results*

When placed in the water on the first trial, each of the seven rats reoriented immediately and escaped through one or other of the open doors with a rapidity undiminished by the passing of thirty-four weeks since the previous experience of the tank. When the doors were first closed, there was some nose bumping but, after two or three trials, the rats were pushing open the door with their former dexterity. On the third day, further evidence of retention was provided by the short time spent struggling at the locked door before retreating. Also on the third day, the six-inch flange was extended to twelve inches for the first five trials on at least one of which each rat scrabbled at that portion of the extended flange which, had it been the customary six inches, would have led directly to the other side. By, on the average, trial 157, all seven rats had reached the learning criterion for the first problem and were started on the second, although they were still, frequently, swimming close to the negative door before turning back from it. The discrimination of two of the rats broke down at once under the new conditions and as, even by trial 400, neither was showing greater than chance success, their training was dicontinued. The other five rats, apparently undisturbed by the change, continued to discriminate and, although they never attained errorless perfection, to increase their percentage of correct trials. Training with two of those five rats was stopped after each had had 1,000 trials (inclusive of some 60 transposition tests); one made almost no errors after trial 90 and the other after trial 130, but neither abandoned one-look discrimination or showed evidence of transposition; their performances, which barely varied over the last 500 trials, involved a sharp right-turn to face the left door which was approached on all trials except if H appeared above it when a smooth deflection brought the rats to the right door (Figure 1*e*). Discrimination training with transposition tests interspersed was continued with the remaining three rats until trial 1,400. These animals will be referred to as Rats II, III and IV respectively.

Rats II, III and IV each showed the same general trends of discriminative and transpositional behaviour. At first, Rats II and III reoriented by means of a sharp left-turn (Figure 1*f*) and Rat IV by means of a right-turn (Figure 1*g*) to look at the left side and approach it or, if H were there, deflect from it to the right. One-look then gave way to two-look discrimination and transposition tests began to give positive results. Rats II and III each performed the looking to and fro while approaching down the centre (as in Figure 1*d*) and Rat IV while swimming down the left side of the tank to the left door (Figure 1*h*). Up to this point, these rats showed the same general trends as Rat I but, after this, their behaviour began to take a different course in that one-look discrimination reappeared and two-look discriminations became fewer until there was a complete reversion to the former which, for each rat, assumed the same form as the original (Figures 1*f* and 1*g*). Various test trials revealed that the discrimination now depended on avoidance of H. At this stage the investigation was closed, for the question of whether two-look discrimination would return was not judged worthy of the labour which seeking its answer would have necessitated. The outcome of the transposition tests with Rats II, III and IV respectively is summarized in Tables II, III and IV. These tables are constructed along the same lines as Table I although the errors, this time, accrued almost entirely from choice of the left door, therefore the results shown are of tests on which the smaller area was on the right side. Omitted from these three tables are the results of those tests made on one-look discrimination (on which the left side was always chosen) for only by eliminating these can transposition's development be clearly seen. Each of the tables shows a tendency for two-step transposition to develop later than one-step transposition.

It was impossible to say that, on such and such a trial, such and such a behaviour pattern ended and another began for, as a rule, there were no sudden transitions; the second pattern might evolve imperceptibly from the first or appear once and not recur for some time. However, the following figures indicate the region where a certain pattern first established its dominance. In Rats II, III and IV respectively: one-look discrimination was stable by trials 320, 200 and 280; two-look discrimination by 770, 500 and 840; one-step transposition by 1132, 629 and 863; two-step transposition by 1277, 1248 and 1143; and, one-look discrimination was dominant again by trial 1360 in all three rats.

IV

DISCUSSION

Employing, as they do, different kinds of motivation and discriminative behaviour, the present set-up and the jumping apparatus invite comparison. Firstly, discriminative learning is much easier in the latter than in the former: lack of an enforced pause before discrimination probably contributes to the difficulty in the swimming situation and, certainly, learning would be easier if the rat's escape activity were more closely centred on the stimuli by having them, not above the doors, but actually on them (Munn, 1950). Secondly, in the jumping apparatus, the choice-point is fixed mid-way between the two stimuli while, in the swimming apparatus, the rat is free to choose at a wide variety of loci. It is this difference which leads (*a*) to the prominence of one-look discrimination in the one situation and its comparative absence in the other, and (*b*) to the greater importance of the negative stimulus over the positive in the swimming situation while, in the jumping apparatus, Maier (1939, p. 324) has found that "the absolute properties of the positive stimulus card are important determiners of what is learned, whereas the absolute properties of the negative stimulus are relatively unimportant." Likewise, Fields (1932), referring to yet another situation where the choice-point is not fixed and where one-look discrimination occurs (p. 27), states that the "avoiding reaction to the negative figure which was found with the running apparatus is almost totally lacking when the jumping apparatus is used"

(p. 31). Additional evidence on those differences comes from an unpublished study, by the writer, of the discriminative behaviour of rats in a modified Grice Apparatus (Baker and Lawrence, 1951; Grice, 1948) in which a hungry rat runs toward the stimuli and pushes its nose through a small, hinged door in the centre of the stimulus to get food. Here, the choice-point is not predetermined and here, again, one-look discrimination is prominent and the negative stimulus is more important than the positive. Thirdly, the water tank provides not only the rigid stimulus control of the jumping apparatus but, also, facilities for observing discriminative behaviour in greater qualitative detail.

Are there any other studies which confirm the present finding that, when relative responding occurs, it is emphasized by further practice? The only evidence, known to the writer, with bearing on the influence of practice on relational responding in rats is reported by Wapner (1944), but hardly assists the present discussion. Using the jumping apparatus, he trained fifteen rats to discriminate between two circles of different size and gave them equivalence tests involving, not size, but brightness differences. Then, he gave them a fourteen-day rest, retrained them on the discrimination and retested them on the equivalence series. The outcome was "an insignificant reduction . . . in the relational responses on the second test compared to the first test" (p. 25). Whether this result was due to the type of stimuli used, to the conditions of training or to the stage of learning at the two periods when testing was carried out, it certainly did not justify the generalization "that retesting results in a decrease of the importance of the relative properties and an increase in the importance of the absolute properties of the training pair" (p. 54).

If there is no further evidence for the practice effect in rats, there is evidence for it in monkeys and in children. Kluver (1933, pp. 129–159) trained two Java monkeys to react positively to a rectangle of 300 sq. cm. and negatively to one of 150 sq. cm. and introduced tests for one-step transposition "at a point when the monkeys had just mastered the problem; at least it cannot be said that they were overtrained" (p. 131). After several days of testing and further training, the monkeys began to show some confusion and the procedure was modified. However, prior to breakdown, groups of test trials were alternated with groups of training trials to produce the results summarized in Table V. In that table, the results for monkeys G.-M. and Id.-R. appear in the first and second rows, respectively. The "L" column shows the number of training trials given at various stages. The "Trans." column is divided into three: "a" shows whether the tests were for upward transposition (U), involving rectangles 300 and 600, or for downward transposition (D), involving rectangles 75 and 150; "b" shows the actual number of trials in the test group; "c" shows the percentage of test trial on which transposition occurred. Each monkey reveals an unbroken trend towards more transposition which is not upset by changes from U-transposition to D-transposition and is, therefore, not dependent on increasing familiarity with the test pair. Also, for each monkey, there is a probability (calculated

TABLE V

L	*Trans.* 1			*L*	*Trans.* 2			*L*	*Trans.* 3			*L*	*Trans.* 4			*L*	*Trans.* 5		
	a	*b*	*c*		*a*	*b*	*c*		*a*	*b*	*c*		*a*	*b*	*c*		*a*	*b*	*c*
545	U	40	67·5	20	U	20	80·0	00	D	20	95·0	10	D	5	100	—	—	—	—
223	U	15	46·7	65	U	30	70·0	20	U	20	90·0	00	D	20	95·0	10	D	5	100

by the chi-square method), of less than ·005 that the degree of transposition in Trans. 1 is the same as that in Trans. 3. The evidence for stronger relational responding with greater practice in children has been reviewed by the writer (Hunter, 1952). There is, then, evidence from three different animal species that, between the time when reaction to relata appears and the time when prolonged overtraining complicates the discriminative behaviour, the effect of practice is to strengthen relational responding.

The writer wishes to thank Professor G. Humphrey for the provision of research facilities and for his interest and stimulation. Professor R. W. Russell's water-tank apparatus at the University of London was the prototype of that discussed here.

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SUBJECTIVE ASSESSMENTS OF FIRMNESS

A COMPARISON OF METHODS USING RUBBER CYLINDERS AND STEEL SPRINGS

BY

D. SHEPPARD and G. W. SCOTT BLAIR

(*National Institute for Research in Dairying, University of Reading*)

Scott Blair and Coppen's work on comparisons of firmness of rubber cylinders by handling has been extended to include steel springs.

Small differences in firmness of rubber cylinders can be detected with considerably greater accuracy than can those for steel springs over the same range. This would seem to be at least partly due to the yielding nature of the rubber surfaces, since covering them with light rigid disks raises the threshold to the level of that of the springs. Moreover, if the ends of the springs are faced with rubber caps, though the firmness of the springs only and not of the rubber caps is varied, a greater acuity is obtained, the threshold lying about half-way between that for uncapped rubbers and that for springs. This suggests that cutaneous cues play a much larger part in such judgments than Scott Blair and Coppen had supposed.

A comparison of simultaneous with successive squeezing methods shows marked differences between subjects, but, on the average, no appreciable difference to threshold. While a single squeeze is inadequate for the best judgments, three squeezings show no advantage and perhaps a slight disadvantage as compared with two squeezings. In the case of steel springs, changing the whole range of firmness within reasonable limits does not alter the position of the threshold curve but, using subjects already familiar with the previous range, there is a marked increase in scatter. Varying the heights of the cylinders produces peculiar and partly unexplained changes in acuity; but in general, taller cylinders are judged relatively firmer than shorter ones. Preliminary experiments using an absolute method suggest the importance of the temperature of the samples and the relative unimportance of even quite long intervals (~ several weeks) between tests, unless experiments on samples having a different range of firmness intervene.

I

INTRODUCTION

Scott Blair and Coppen (1939 *a* & *b*) have studied the assessing of differences of firmness when pairs of rubber cylinders of height 2·5 cm. and diameter 2·0 cm. were simultaneously squeezed between fingers and thumb in the two hands. Subjects were allowed to squeeze each pair only once before changing the cylinders from one hand to another. This change was made twice, each pair being squeezed three times before reporting on which sample of the pair was the softer. "Indistinguishable" judgments, though discouraged, were allowed, and distributed equally in the scoring between the "correct" and "incorrect" categories.

The rubber cylinders were made so as to differ in firmness (n) and their "firmness" was objectively determined as the reciprocal of the compression produced under an arbitrary static load.

It was found that curves plotting percentage of correct answers (p) against log $\triangle n$ were approximately linear over a considerable range of firmness, $\triangle n$ being defined as [(deformation of softer cylinder under load − deformation of firmer cylinder) ÷ deformation of softer cylinder] × 100.

These straight lines or "threshold curves" were compared with similar curves using viscous fluids of high viscosity and, in later papers, with more complex materials (Scott Blair and Coppen, 1940, *a* & *b*, 1942, 1943), since the purpose of all these researches has been ultimately to study judgments of certain aspects of quality of cheese and other dairy materials. A more satisfactory statistical analysis of the data was later made by Harper (1947; 1949) using a number of methods, including Probit Analysis.

Scott Blair and Coppen compared threshold values (generally at 80 per cent. levels) for subjects of different sex, age, training, occupation, etc., but were well aware that rubber was not so simple a physical system as was desired. An ideal elastic solid would compress immediately under constant load by an amount strictly proportional to the load and should recover completely and immediately when the load is removed. Rubber is far from behaving in this simple way, nor is any other composite material known which would do so. Rubber and all other high-elastic materials also have the serious disadvantage that their properties change rather quickly as a result of ageing.

In the present paper, similar experiments are described using a series of eight helical steel springs differing in firmness. These were faced with rigid "plastic" end-disks and bound with tape: the springs in any given series were of the same weight and were visually indistinguishable except for a somewhat elaborate system of code numbers marked on the tape. The samples had the same overall dimensions as had Scott Blair and Coppen's rubber cylinders.

The surface of these samples, unlike those of the rubber cylinders, did not yield to the pressure of the fingers and thumb. This was not expected to be important, since Scott Blair and Coppen had come to the conclusion that judgments on rubber cylinders depended much more on kinaesthetic than on cutaneous cues, a conclusion which the results of the experiments now to be described will seriously challenge.

There are several reasons for suggesting this. Since the amount of joint movement is likely to be similar for both springs and rubbers, and since the firmness ranges are much the same, it would seem that the differences in acuity are most probably due to the different nature of the yielding of the surfaces. There are also certain differences in the behaviour of rubber cylinders and springs, in that the former cannot be deformed quite as swiftly as the latter, and also that the rubbers may twist sideways when they are squeezed, whereas the springs seldom do this, or only very little.

Despite this, altering the nature of the surfaces seems to bring about great changes in acuity (*see* below). It seems therefore, as if the subjects can get some idea of the firmness of the cylinders by the "feel" of a yielding surface, and when they have to judge from deformation alone, they cannot detect such small differences. Introspections tend to confirm this conclusion.

Normally the light touch receptors would not help much in such judgments, since they adapt very quickly (Fulton, 1943; Adrian, 1934). Here, however, the time taken in squeezing a cylinder is so short, and the load applied so intense (about 3 Kg/cm^2) that adaptation is probably not important. There is a further point in favour of this in that certain subjects who are particularly good at detecting small differences in firmness, when the surface is of such a nature that it will yield, base their judgments on a very light "feeling" of the surfaces.

It would be dangerous to suggest, however, that the light touch receptors, or even those for deep pressure, are of more importance than are the kinaesthetic receptors in these judgments, but it seems reasonable to suggest that they are important as accessory cues.

Sullivan (1927) has shown that the feeling of hardness in materials screened from view is conditioned by the presence of a definite boundary line and also that the same objects tend to be judged soft when warm and hard when cold. She claims that it is almost impossible to perceive a cold body as soft and only possible to perceive a warm body as hard when it is marked by clear boundaries.

II

EXPERIMENTAL METHODS

Method of Paired Comparisons

The experimental methods differed somewhat from those of Scott Blair and Coppen. In the main experiment, in each test, all twenty-eight possible pairs of the series of eight cylinders were presented to the subjects in a randomized order which was not changed throughout the series of experiments. The order of presentation within each pair was separately randomized for each experiment, but in such a way that equal numbers of objectively "firmer first" and "softer first" pairs were given.

The subject was asked to return first whichever of the pair of samples seemed firmer, giving any comments such as "firmer pair," "softer pair," "definite," "wild guess," etc., but was not allowed to report "indistinguishable." This restriction seldom appeared to upset the subject (Fernberger, 1914). The subject was also allowed to give only a single squeezing while the sample was in the hand. Several methods were used:

(*a*) *Simultaneous methods* (Σ). The sample presented first is taken in the right hand and the second in the left hand. This method is subdivided into three groups, one using a single simultaneous squeeze (Σ_1), another with two squeezings, changing hands once (Σ_2) and a third with three squeezings (the method used by Scott Blair and Coppen), changing hands twice (Σ_3).

(*b*) *Successive methods* (κ). The subject uses the preferred hand and squeezes the two samples consecutively in the order presented; this is also subdivided into two methods, using one squeeze on each sample (κ_1) and using two successive squeezes (κ_2).

In order to compare differences in absolute ranges of firmness, as distinct from Δn, two series of springs were used; a firm series (range of compression from 1·6 to 2·3 mm. per 5 kg.) and a softer series (from 2·3 to 3·6 mm.). The numbers of subjects and total numbers of tests varied for the different methods.

For comparison with the results on steel springs, a new set of rubber cylinders, very similar to those used by Scott Blair and Coppen, was obtained from the Research Association of British Rubber Manufacturers.

These were used both in their natural state and also with rigid plastic disks fitted on their ends. This process alters somewhat the behaviour of the cylinders under static load. The objective firmness was therefore re-determined in each case. The weights of the rubber and steel samples were very different and experiments on the effect of such differences have not yet been made. The plastic caps increased the height and area slightly: the effects of height differences have been separately studied and will be discussed later. Here it suffices to say that the effect on the threshold curve is likely to be insignificant.

"Blind Selection" (Absolute) Method

In a subsidiary series of experiments, known as "blind-selection tests," one of the experimenters who had frequently handled the spring samples and knew the code numbers, would select, with closed eyes, one of the samples. He would squeeze this once or more times according to the experiment, guess the number of the sample and then record both the guess and the correct answer. Fifty such "blind selections" normally constituted a single experiment. The algebraic and arithmetic means of the errors in Δn were calculated. There was a general tendency to judge all the cylinders too firm, but this was considerably influenced by the temperature of the caps (cf. ref. to Sullivan above). Mental standards were quickly established but easily upset if a series of springs having a different range of firmness was tested between the experiments. Quite long intervals between tests, without the use of other ranges of springs, had very little effect.

Apart from the effect of the temperature of the caps, it was found that any tendency to judge the cylinder as either too firm, or too soft, was emphasized when the time-interval between each judgment was extended.

Contrast effects were noted in that the judgment of the firmness of any cylinder tended to be affected by the firmness of the preceding cylinder. For example, a cylinder of average firmness would be judged softer than its real value if presented after a very firm cylinder, and correspondingly firmer if presented after a very soft cylinder. Experiments using this and other absolute methods will be more fully described in a later paper in this series.

III

RESULTS OF THE TESTS

Main series of experiments:

The experiments may be classified as follows:

Experiment No.	1	2	3	4	5	6	7	8	9	10
Samples	Springs (firm)	Springs (firm)	Springs (firm)	Springs (soft)	Springs (firm)	Springs (firm)	Rubbers	Rubbers	Rubbers (capped)	Springs (rubber caps)
Method	Σ_1	Σ_2	Σ_3	Σ_3	κ_1	κ_2	κ_1	Σ_3	Σ_3	Σ_3
Number of tests	56	30	30	40	169	30	35	48	38	35
Number of subjects ..	17	6	4	12	>16	5	7	10	6	9
Δn for 75 per cent. correct level	24	14	17	16	24	14	11	9	18	13

Total number of tests: 511

The scatter curves were plotted on probability paper, and four of them are shown in the figure (circles). The 28 pairs were averaged into five main groups and are represented by the crosses. There is no reason to suppose that the distribution is fully Gaussian and judgments may well be affected by physical stimuli other than Δn. For these and other reasons a Probit Analysis was not made. Since certain individual points fall below the chance level at 50 per cent. correct, not only these but all individual points below the 55 per cent. correct level are omitted. There is no satisfactory way of calculating best straight lines through the points, so these were drawn in by eye through the crosses, attempting to weight in favour of the middle part of the curve. The values of Δn interpolated for a 75 per cent. correct level are given in the table, since, using a "probability" plot, interpolation at the more usual 80 per cent. level would be less accurate.

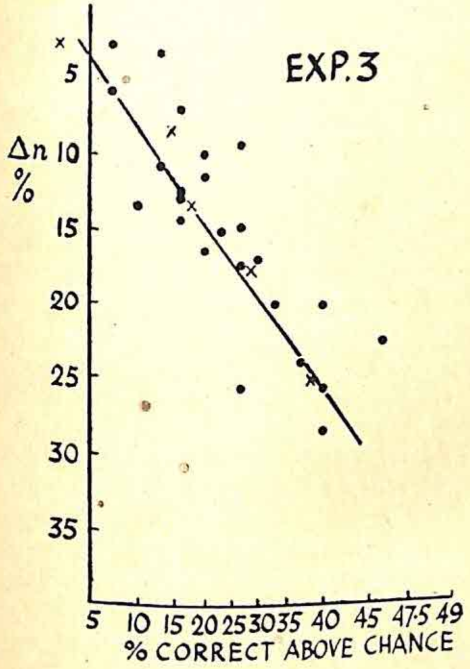
IV

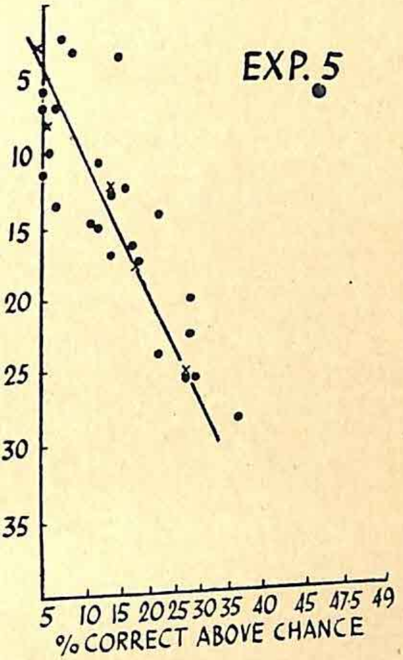
DISCUSSION OF RESULTS

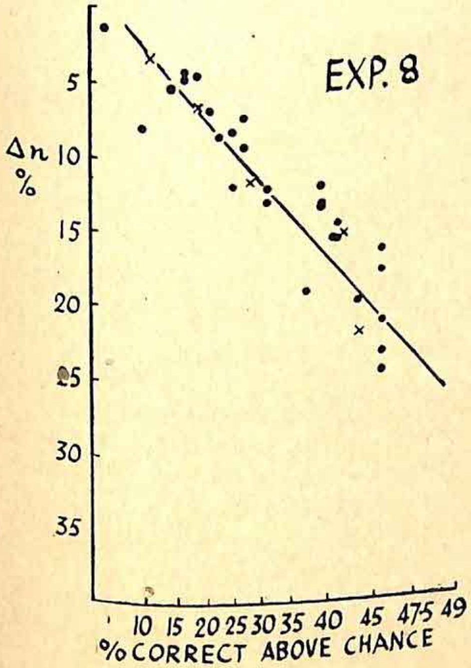
The most striking result of this main series of experiments is the very much greater sensitivity obtained in comparing the rubber cylinders (uncapped) than that obtained with steel springs using the same methods. But this greater sensitivity is immediately lost if the rubber cylinders are "capped" so that the surfaces cannot yield under the pressure of the fingers and thumb.

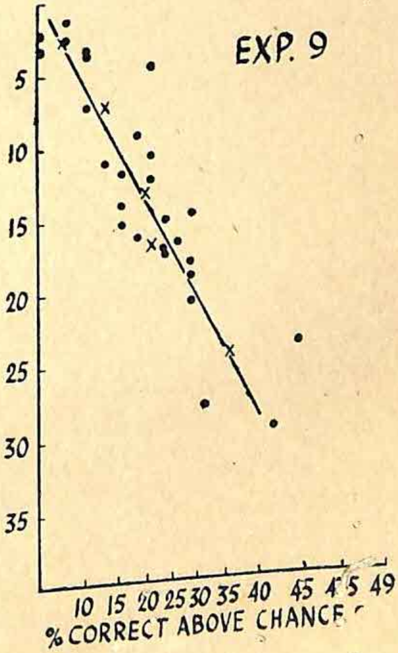
This strongly suggests that cutaneous sensations are much more important than Scott Blair and Coppen believed, their views being mainly based on the (negative) results of some experiments on the elastic hysteresis and semi-permanent deformation of finger-tissue under progressively increasing and decreasing loads.

Since no statistical test for significance is readily available, conclusions that one method is relatively better than another can only be made when there are large differences in the threshold levels. The lowest threshold is clearly that for uncapped rubbers, Δn (75 per cent.) being 9 per cent. for method Σ_3 and 11 per cent. for κ_1. Capping the rubbers (method Σ_3) raises the threshold to 18 per cent., which does not seem to differ significantly from the value of 17 per cent. for the same method for springs. Using a different range of firmness for the springs, method Σ_3 gives a value of 16 per cent., which is unlikely to be significantly different. Methods Σ_2 and κ_2 are probably the best with thresholds of 14 per cent. each, single squeezing methods, Σ_1 and κ_1, being somewhat higher at 24 per cent. There would seem to be no advantage in using three squeezings. Though not at present verifiable statistically, it seems probable that small differences in $\triangle n$ are better detected with a single than with a double squeeze. The threshold curves would seem to cross. This is not apparent from the figure because of the omission of data when $p < 55$ per cent. There is, on the average, no observable difference between simultaneous (Σ) and non-simultaneous (κ) methods for springs for the same number of squeezings, unless perhaps for small differences in $\triangle n$.









The Effect of Fitting Rubber Caps on the Ends of the Springs

In order to see whether the threshold for springs, using method Σ_3, could be improved by making the end surfaces of a yielding nature, rubber caps, all made of the same rubber and therefore of the same firmness, were fitted on to the ends of the springs. This had the effect of reducing considerably the range of $\triangle n$ and one of the cylinders which had previously been third in objective order of firmness, for some unknown reason moved into first place. Subjective judgments seemed to depend rather on the properties of the spring than on those of the complete cylinder, since the percentage correct answers on 35 runs in comparison with the two next firmest cylinders were as low as 34 and 37 per cent.

Perhaps on account of the small range of $\triangle n$, the experiment showed a wide scatter; nevertheless it was possible to get a fair estimate of the $\triangle n$ level for 75 per cent. correct judgments, giving a value of about 13 per cent., i.e. approximately half-way between the value for rubber cylinders and that for either springs or rubbers fitted with rigid caps.

Thus it would seem that the yielding of the surfaces of the cylinders did increase acuity somewhat, even though the amount that they yielded was independent of the firmness of the cylinder as a whole.

Variation in Heights of Cylinders

In one series of tests, the thickness of the caps of the cylinders varied in height, total heights being 2·3, 2·5, 2·7 and 2·9 cm. as distinct from the standard 2·5 cm. and the weights being equalized. In all cases, the diameter was 2.0 cm.

There are three ways in which a subject can set about comparing the firmness of two springs: (*a*) He may try to squeeze with the same pressure and judge by the amount of deformation produced, (*b*) he may attempt to produce a constant deformation and judge by the reaction-pressure on the fingers, (*c*) he may make no conscious analysis, just squeezing and giving a spontaneous judgment. This is a sort of combination of (*a*) and (*b*).

Scott Blair and Coppen definitely encouraged their subjects to use method (*a*); but as a rule, (*c*) gives the best results, and subjects are now left entirely free to use their own methods.

If method (*a*) or (*b*) is used it is of interest to enquire whether the subject judges by absolute or relative deformation. Thus, if two identical steel springs are built up with caps to different heights, a load which produces 1 mm. compression in both, will produce a 5 per cent. compression if the cylinder is 2 cm. high, but only $3\frac{1}{3}$ per cent. if the cylinder is 3 cm. high.

In fact it did not seem advisable to vary heights by such large amounts, but when smaller variations were made, the results were quite striking and remain partly unexplained.

Perhaps it is not surprising that giving cylinders of mixed heights lowered the general level of discrimination. Almost all subjects complained that they found it difficult to keep adjusting their height standard. Nor is it very remarkable that taller cylinders tended to be judged as relatively firmer than shorter ones. This could be explained either in terms of relative deformations, or by suggesting that the slightly greater leverage needed to handle the taller cylinders would result in the unconscious application of smaller pressures.

The most remarkable result, however, was that the main fall in discrimination occurred not in the pairs differing in height, but in those having the same height. Thus errors in pairs having the same height and assuming that the chances of a correct judgment are proportional to $\triangle n$, were found to be 149 per cent. of the chance value.

With a difference in height of 2 mm. the figure was 93 per cent.
,, ,, ,, ,, ,, ,, 4 mm. ,, ,, ,, 92 ,, ,,
,, ,, ,, ,, ,, ,, 6 mm. ,, ,, ,, 95 ,, ,,

The reason for this is not known.

One pair of cylinders, differing in height by 2 mm. showed no measurable difference in firmness. In this case, judgments of firmness difference depended partly on the order of presentation, but more on the height difference.

Thus	1st taller	1st shorter	2nd taller	2nd shorter
Number given as firmer ..	15	7	10	4

Method of Handling Cylinders

Some subjects held the cylinders between the tip of the index finger and the ball of the thumb: others used the side of the index finger at about the second joint. Some squeezed slowly, some fast; some hard and some gently.

A number of experiments was done forcing subjects to change their methods, either with respect to position, pressure or tempo. In almost all cases the subject's natural procedures were found to be best.

Subjects were normally not told whether their answers were correct until the end of the test. They were encouraged to guess their overall score and then informed of the actual result.

A few subjects were also told the results of each pair before being given the next pair. This did not generally effect any great improvement.

"Handedness"

Left- and right-handedness are far from simple factors (we are indebted to Mr. B. Babington-Smith, of Oxford, for discussions on the question). Nevertheless, the majority of our subjects were in most senses right-handed and a minority were especially selected as left-handed. In many cases right-handed subjects made as good a score when forced to use the left hand in κ-experiments. In a few cases, left-handers did as well with their right hands.

An attempt was made to relate scores to cross-lateralism (i.e. right-eyed left-handers and left-eyed right-handers), but so far, results have been inconclusive.

A particularly interesting study of "handedness" was made in the case of the Σ_3 rubber cylinder experiment. (A somewhat similar experiment was also done using method Σ_1). In every case, the first cylinder of each pair presented was squeezed twice in the right hand and only once in the left. Among the errors made by eight right-handers, each subject showed a characteristic tendency either to give the sample squeezed twice in the right hand as firmer or to give that squeezed twice in the left. The number of pairs in which the sample judged wrongly as firmer was that squeezed twice in the right hand, was calculated as a ratio of the total number of errors. Averages of these ratios varied for individual subjects from 78 to 11 per cent. Of the two left-handed subjects, one gave 76 and the other 19 per cent. It is thus clear that any effects of handedness are quite overruled by differences in compensation This was not so, however, in the Σ_1 experiment, in which all the left-handed subjects had ratios of less than 50 per cent., except when the test was artificially speeded up.

V

FUTURE WORK

When a cylinder fitted with rigid caps is squeezed between the fingers, the ends of the cylinder do not in any way conform to the shape of the fingers. With rubber, there is some conformity, but this is by no means complete. With a true fluid of high viscosity (such as Californian bitumen), the material will conform perfectly to the deforming bodies, provided enough time is given. It is intended to repeat Scott Blair and Coppen's work on true fluids but facing the fluid cylinders with rigid caps. The way in which steel springs and rubber cylinders deform, differs comparatively slightly; whereas cylinders of a truly fluid material behave quite differently, since the material flows at a steady rate under constant pressure and at a rate proportional to the applied pressure when this is varied. Such experiments should throw further light on the relative importance of cutaneous and kinaesthetic sensations in defining acuity.

An interesting question, at present quite uninvestigated, is the significance of differences in the weight (density) of the cylinders. Rubber and fluid cylinders do not differ greatly in their weight; steel springs are much heavier. This point requires further investigation.

It is hoped also to measure the pressures actually exerted by the fingers and to examine more carefully the relative importance of the part played in the judgment by cutaneous and kinaesthetic cues. A set of tests has been devised for this purpose, and it is hoped they may help in deciding whether subjects base their judgments mostly on differences in deformation when applying a constant load, or by comparing the loads required to produce a constant deformation.

More research needs to be done on the question of the accuracy of absolute judgments, and the possibility of their being maintained adequately over periods of time.

Our best thanks are due to Professor R. C. Oldfield for much advice and encouragement; to Mr. C. P. Cox and to Miss S. Mayne for consultations on statistical treatment; to all our subjects; and especially to the Director of the Commonwealth Bureau of Dairy Science for permission to use members of his staff as subjects.

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A PENDULUM PHENOMENON IN THE VISUAL PERCEPTION OF APPARENT MOVEMENT

BY

K. R. L. HALL, A. E. EARLE and T. G. CROOKES

(*From the Department of Psychology, Barrow Hospital, Bristol*)

In setting up an apparatus for studying the phenomenon of apparent movement it was noticed that, when a metronome was employed for making the electrical contacts in the light-circuits, the path of apparent movement between the two lights was curved instead of straight. This deviation could not be attributed to the presence of any distorting structure in the visual field, and an experimental investigation of the conditions of the phenomenon was begun. So far five different display conditions have each been observed by ten or eleven subjects individually. No subject saw more than one condition in the experimental series, and their reports indicate that the following factors, in order of importance, are effective in leading to the perception of curved apparent movement: (1) the shape of the light-stimuli, (2) the regularity of the rhythm of presentation, (3) the gradient of brightening and dimming of the lights and (4) the sound of the metronome in synchrony with the appearance of the lights.

The possible role of past experience in the perceptual process, and the relationship of this "pendular" phenomenon to Johansson's (1950) "wandering" phenomenon and to normal stroboscopic movement, are briefly discussed.

I

INTRODUCTION

While preparing an apparatus for studying individual differences in threshold for the phi-phenomenon it was noticed that the apparent movement between the two lights seemed to be taking a definitely curved track on the vertical surface of the screen, and the "light" sometimes appeared to be swinging or rocking with a pendulum-type of motion. It was at first thought that this might be related to the fact that a metronome was being used to make the alternate electrical contacts for the two lights and experiments were accordingly devised to test this hypothesis, and to discover more precisely, by systematic variation, under what conditions such a phenomenon occurred.

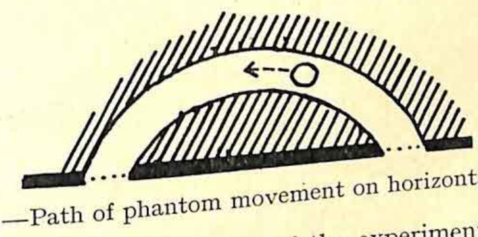

FIG. 1.—Path of phantom movement on horizontal plane.

After completing the first three stages of the experiment described below, our attention was drawn to a paper by Ekman (1951) describing what appeared to be a somewhat similar phenomenon observed by Johansson (1950). In Johansson's basic experiment the display consisted, in effect, of a vertical wooden screen with two frosted windows 1·5 cm. in diameter and lying 5 cm. apart on a horizontal axis. A mechanical device caused the brightness of two flashlamp bulbs to vary harmonically at 0·5 c/s and in antiphase. His five trained subjects, carrying out their observations in a normally-lit room, interpreted the display not as consisting of two lights of varying intensity, but in terms of a single light-source moving in a horizontal curve behind the screen and between the two windows. Sometimes the light was thought to be moving to and fro in a semi-circular canal behind the screen. Figure 1 taken from Johansson (p. 149) illustrates, in plan view, the supposed path of movement of this "phantom" source of light.

The phenomenon is also sometimes interpreted as a light attached to a pendulum swinging slowly in the space at the back of the screen, and having a calm, even, gliding motion. Always, however, the swinging, semi-circular motion seems to take place in a horizontal plane, and there is no record, from his experiments, of subjects reporting movement in any other plane. Johansson called the phenomenon which his subjects observed the "W" or "Wandering" phenomenon, in order to distinguish it from ordinary stroboscopic movement.

As Johansson is careful to point out, his subjects are almost always well-trained in phenomenological description, and are instructed simply to look at the screen in front of them and report naïvely what they see.

Having described the basic phenomenon, Johansson goes on to investigate certain conditions under which it occurs. He found that it still continued to be seen with phase-differences of 45° and 90°, with the lights more widely separated, and with an interrupted rhythm. Further experiments showed that a similar experience of "phantom" movement can be made to occur in other sense modalities. For example, two earphones, giving diotic stimulation, with stimulus-intensity varying harmonically and in antiphase, produced the experience of a source of sound moving backwards and forwards between the two ears, either over the head, following the ear-phone mounting, or else actually straight through the head. With vibrators of similarly varying intensity held one in either hand, a "phantom" source of vibration wanders from one hand to the other in a to-and-fro motion, one subject describing the source of vibration as moving along a curved path that dips downwards between the vibrators.

As already mentioned, in our early observations the movement phenomenon had appeared as definitely taking place in the vertical plane, and we set out initially to determine under what conditions such swinging, pendular movement would appear and under what conditions we should produce straight movement. Johansson's experiments suggested further variations, which will be described in detail in the following sections. One point in particular in Johansson's results seemed open to criticism, and that was his use, under several different experimental conditions, of the same few well-trained subjects. Speaking as a result of our own observations of the phenomena under a variety of conditions, there is no doubt that one can, to some extent, see into the phenomena what one expects to see. We have therefore used completely different subjects for each of the main experimental variations, and only use our own observations and those of "predisposed" observers to supplement the unbiased observations of the main groups.

II

METHOD AND PROCEDURE

The subjects have been University students, doctors, nurses, administrative and laboratory staff, of both sexes. The majority come within the age range 20–35.

Apparatus

The display unit consisted of a wooden box, one side of which contained a panel of frosted glass 12 in. × 6 in., this serving as a screen. Inside the box a metal rod ran horizontally from side to side, near the top, and parallel to the screen. Two clamps, each supporting a flashlamp bulb-holder, were fitted to the rod, this arrangement making it possible to bring the window-end of each bulb-holder into contact with the inner surface of the screen in any chosen position. The bulb-holder windows contained translucent material, and their shape could be either circular (diameter 1·25 cm.) or square (0·8 cm.). A lid enabled adjustments in the positions of the lights to be made.

In order to produce stimuli of equal duration occurring alternately in either bulb, and with an equal interval of time between each stimulus, a metronome fitted with mercury-cup electrical contacts was introduced into the lamp-circuit, which also contained a variable resistor in series for adjusting the brightness of the bulbs. The metronome was set so that it connected the battery to either lamp for 220 m/s., the time interval between successive contacts being 110 m/s., and the total event of Light 1 + Interval + Light 2 = 220 + 110 + 220 = 550 m/s.

Procedure

For all five conditions of the experiment, the method of presentation and instruction to the subject has been kept constant. The subject was seated 6 ft. away from the screen placed approximately at eye-level on a table. The screen itself and the rest of the apparatus was covered from view until the experiment began. Experimental observations were always carried out in a darkened room. Instructions were: "I want you to look at this screen in front of you which I shall uncover. When I turn the room light out I want you to give me your impressions, in the form of a running commentary, of what seems to you to be happening on the screen." No indication was given as to what the subject might expect to see. In the course of the observation period, the experimenter occasionally asked a subject to elaborate on some point, but great care was always taken not to make any suggestion which might influence the perceptions of the subject. All verbalization was recorded verbatim. At the end of an observation period, the subject was asked, wherever necessary, to draw the path of any movement seen, to add any further comments, and to match his impression of the amplitude of any curved movement seen from cards on which were drawn arcs varying in steps from "very shallow" to "very deep."

III

The Experiments

Condition 1. In this condition, the metronome, although never visible to the subject, was clearly audible, being set immediately behind the light box, auditory rhythm and light rhythm thus being exactly coincident. The shape of the lights was circular. This rhythm frequency was kept constant, while three different distances apart were used, so that the subject saw the lights at 18·5 cm., at 9·5 cm. and at 6·5 cm.

TABLE I

Frequency of Four Categories of Movement Perception for Ten Subjects under Condition 1

Distance apart in cm.	*Vertical curve*	*Horizontal curve*	*Straight*	*Two-light movement*	*Total movement*	*Total no-movement*
18·5	5	0	3	1	9	1
9·5	9	0	1	0	10	0
6·5	9	0	0	0	9	1
Total ..	23	0	4	1	28	2

This is the original condition under which the experimenters themselves had observed the pendular effect. Results from ten subjects substantially confirmed our original observations as to the curved path the light was taking. A summary of these results is shown in Table I, where the frequency with which four different categories of movement were seen at the three distances apart is shown. The first category

(Vertical curve) is where the subject sees the light moving in a curved, pendular path across the screen on a vertical plane. The second category (Horizontal curve) is where the movement of the light is seen as taking place in a curved or circular track out from and behind the screen on a *horizontal* plane—as in Johansson's experiments. The third category (straight) is where ordinary stroboscopic movement occurs, and the fourth category is where there is some sort of movement of each of the two lights, rather see-saw like, but no continuous movement of one light across or behind the screen.

It will be seen that the vertical-curve type of movement perception is by far the most frequent. The phenomenon is described in a variety of ways, examples of which are: "At first, I saw two lights, then very soon one light swinging on a pendulum," this subject drawing the light path as downwards, below the horizontal, and describing it as always on a vertical plane, i.e. on the screen itself; "one light moving like a pendulum, or like a man on a railway swinging a lantern"; "mostly an arc-like movement upwards, but sometimes the movement goes horizontal."

The most common description is with reference to the pendulum effect. This is usually seen as a downward curve, but occasionally, as in the last example, the movement is seen taking place above the horizontal—that is to say, it follows the same path as the metronome itself. In a few instances, the movement is seen as a complete ellipse occurring on the face of the screen. In several instances, subjects report seeing both vertical-curve and ordinary straight movement during the same observation period. Once the pendular effect is observed, however, it tends to become the predominant perception so that such observations have been placed under the vertical-curve heading.

The actual sequence of observations for one subject is of some interest as showing the development of the pendular effect. At 18·5 cm., he reported: "A see-saw movement, like the ends of a see-saw tipping up and down," i.e. each light was moving separately up and down, and there was no single path across the screen. At 9·5 cm., he reported: "Like a swing, moving up and down, but incomplete." Here again each light was said to be describing an up-down path, but now merging towards each other. He drew the tracks of the lights as shown in Figure 2 (1).

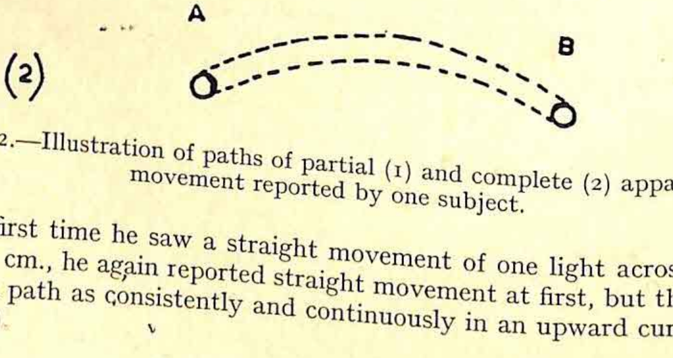

FIG. 2.—Illustration of paths of partial (1) and complete (2) apparent movement reported by one subject.

Then for the first time he saw a straight movement of one light across the screen. Finally, at 6·5 cm., he again reported straight movement at first, but then perceived the movement path as consistently and continuously in an upward curve, as shown in Figure 2 (2).

It appears, from questioning subjects after the experimental session, that few, if any, of them made any conscious interpretation of the light-phenomenon in connection with a metronome or other mechanical device.

Condition 2. As already stated, it seemed possible that this pendular effect was being produced by the coincident auditory rhythm of the metronome. The fact that most subjects saw the movement as downward, following upon a clock-pendulum model, suggested that it might be the rhythmic sound itself, rather than the particular device producing it, which was influencing the movement perception.

The next stage, therefore, in the investigation was to find out whether the same effect occurred if the metronome was no longer audible to the subject, although it was still being used to produce the identical visual situation used in Condition 1. Accordingly, all the conditions of the experiment of Condition 1 were kept the same except that the metronome was now put in a room away from the visual apparatus so that it was completely inaudible to the subject. Great care was taken that the subject should receive no indication as to what device was producing the light contacts.

Results for ten subjects indicate that the perception of a pendular or arcuate type of movement does not depend primarily on the parallel auditory stimulus provided by the metronome rhythm. A summary of the subjects' reports, classified as in Table I, is given in Table II below.

TABLE II

FREQUENCY OF FOUR CATEGORIES OF MOVEMENT PERCEPTION FOR TEN SUBJECTS UNDER CONDITION 2

Distance apart in cm.	*Vertical curve*	*Horizontal curve*	*Straight*	*Two-light movement*	*Total movement*	*Total no-movement*
18·5	1	0	1	0	2	8
9·5	6	0	2	1	9	1
6·5	5	2	3	0	10	0
Total ..	12	2	6	1	21	9

Comparison of these observations with those made under Condition 1 shows several features of some interest. First, there is *less* vertical curve movement at the widest distance apart than where the metronome is audible. This would seem to indicate that the metronome has some reinforcing effect upon the visual perception where the visual field is relatively unstructured, that is to say, where the conditions may perhaps be considered as midway between light succession and continuous movement. Secondly, there is more straight movement perception at the 9·5 and 6·5 cm. distances than under Condition 1, and also there are indications that the pendular movement, when it is perceived, is seen as *shallower* than under Condition 1. This is shown by the subjects' reports of "slight curve," and by the fact that, when they were asked to choose from a set of arcs drawn on a card and varying progressively from deep to very shallow and almost straight, they tended to choose from the "very shallow" end of the series. A third observation indicating that the metronome beat may have only a secondary effect upon the movement perception is that here, for the first time, we have two reports of a phenomenon similar to that observed by Johansson's subjects. At the 6·5 cm. distance, one subject reported that the light was moving circularly backwards, "like a pendulum, curved backwards." He then elaborated on this by saying that it was describing an arc somewhere on a plane between the horizontal and the vertical, and "not in an arc at right angles to me." Another subject, at the same distance, and having previously reported vertical-curve movement, said: "It depends on how you think of it. It can either be rocking in one place, or in a semi-circle through the apparatus and back—on the horizontal."

From these results, it would appear that the plane in which the circular movement is perceived is affected by the presence of the metronome, and that the depth of the pendular effect is strengthened by the auditory stimulus, but that some sort of arcuate movement is still seen by six subjects at 9·5 cm. and seven subjects at 6·5 cm. Statistically, the number of vertical curve observations made under Condition 1 is not quite significantly greater at the ·05 probability level than under Condition 2 (chi² = 3·675).

Condition 3. It now became necessary to carry the investigation a stage further in order to find out more precisely under what conditions the pendular-type of perception occurred. Accordingly, it was decided to see what effect the shape of the light itself might have upon the movement perception. Under this condition, therefore, the outline of the lights appearing on the screen was made square instead of circular, the base of the square being exactly parallel with the horizontal line of the screen, the sides of the square being 0·8 cm. Except for this variation, all the conditions of the experiment were identical with those for Condition 1, the metronome being present in the room.

Here, the results are quite conclusive, as can be seen from the summary of observations in Table III. Only in one case, at 6·5 cm., is there any report of vertical curve. There are no reports of horizontal curve, and 19 reports of straight movement. The proportion of vertical curve movement seen under this condition is highly significantly lower than under Condition I (chi² = 30·66, for which $P = < \cdot 001$).

TABLE III

FREQUENCY OF FOUR CATEGORIES OF MOVEMENT PERCEPTION FOR TEN SUBJECTS UNDER CONDITION 3

Distance apart in cm.	*Vertical curve*	*Horizontal curve*	*Straight*	*Two-light movement*	*Total movement*	*Total no-movement*
18·5	0	0	4	1	5	5
9·5	0	0	8	0	8	2
6·5	1	0	7	2	10	0
Total ..	1	0	19	3	23	7

It will be seen from the Total Movement frequencies of all three tables that the total number of movement perceptions does not vary greatly under the three different conditions. What does vary significantly is the character of the movement perception itself. Quite clearly the path of the movement is greatly influenced by the shape of the light itself. The *circularity* of the lights used under Conditions 1 and 2 would seem to provide a major factor influencing the kind of movement observed.

Particularly interesting here is the evidence of perceptual compromise between the visual and the auditory rhythm background stimulation which occurred in three cases. One subject never saw any connection between the two lights, so that there was never any movement across the screen. At 6·5 cm., however, his description was as follows: "It is as if a square light were on all the time, but screened by shutters. These open with vertical movements up and down, exposing the light—first one light, then the other." Later, this subject reported that he had had a clock at home which had a pendulum which moved vertically up and down just as the lights seemed to do in the present experiment. A second subject reported a very shallow arcuate movement at 6·5 cm.: "I make myself see it as an arc—maybe because I'm thinking of what is inside the box—if it's a metronome, it's upside down." This is the only report of vertical curve movement made in this group of ten subjects. This subject reported that he had imagined the light at first as moving in a straight line, so that the slight arcuate movement perceived later would seem to be due to his attempt to relate the actual visual perception to the secondary background phenomenon provided by the metronome. Finally, a third subject reported that the light was moving to and fro like a pendulum, but in an absolutely straight track with no suggestion of curved motion.

Four of the subjects who had seen the lights squared as for Condition 3 were subsequently shown the lights under Condition 1 (circular, with metronome). Two of these now saw the movement as definitely pendular, on a vertical plane; one continued to see it as always a straight movement, and the fourth (the same subject as reported the two-light shutter movement), still saw the phenomenon in terms of his shutter model, but now also with a definite rocking motion occurring towards the centre.

Similarly, five subjects who had seen the phenomenon under Condition 1 were now shown it under Condition 3, with the squared lights. Two reported a swinging, pendular motion throughout, just as they had done under Condition 1. The remaining three said that they were able to see the movement as either curved vertically or straight, although they indicated that the curved movement was now shallower than previously.

These subsidiary results obtained from nine subjects in retesting under a different condition are of value mainly in showing that it is desirable in an experiment of this kind to use different groups of subjects for each condition.

Condition 4. It was at this stage of the experiment that Johansson's experiments describing the "W" phenomenon came to our attention. It will be recalled that his subjects reported an impression of a source of light moving along a horizontal curve when they observed two lights the intensities of which were varying harmonically and in anti-phase. Our display differed from that of Johansson in the following ways:—

In our apparatus, only the lights were visible to the subject; in Johansson's the surface of the screen was also visible.

In our apparatus, an interval of time occurred between the appearance of the two lights, the two never being on together. In Johansson's, both lights were visible continuously at some degree of brightness.

Nevertheless, it seemed to us that the gradient of brightening and dimming of our lights, caused by the warming and cooling of the bulb-filaments, might be producing a type of harmonic variation in stimulus intensity which, though cut-off and distorted, could be a contributing factor to the perception of curved movement.

Accordingly, we decided to examine this aspect of the problem further by using neon lights with an almost instantaneous on-off exposure in place of the ordinary flash-lamp bulbs used in the previous conditions. The lights were circular and 1·25 cm. in diameter. Temporal conditions were exactly as for Condition 1. The neons gave a reddish-coloured light, but, when shaded flash-lamp bulbs, matched for colour and intensity with the neons, were shown to three subjects, all of them reported curved movement.

For mechanical reasons, the distance apart of the two lights was set at a point between the two distances for which the greatest number of movement perceptions was reported under conditions 1–3 (i.e. between 9·5 and 6·5 cm., actual distance being 7·75 cm.).

For eleven subjects, five reported a main impression of swinging, pendular type of movement apparently always in the vertical plane, and always of shallow amplitude. The other six subjects saw completely straight movement backwards and forwards across the screen. In comparison with Condition 1, this difference is not quite significant at the ·05 probability level, chi^2 being 3·594 when Yates' correction for continuity is applied. It seemed, therefore, that, although the brief warming-up and cooling of the flashlight bulb filaments under the other conditions must be one factor contributing to the vertical-curve movement perception, it is, as we had supposed from our own results, only one of several contributory factors. Further, our results for this condition indicate that, for our own display unit, the anti-phase intensity variation is probably only a minor factor, the shape of the light and the rhythm itself, whether auditorily reinforced or not, being at least as important.

Condition 5. The final variation consisted in disrupting the continuity of the rhythmic presentation of the lights, in order to observe how important the rhythm itself might be in influencing the type of movement perception. It was therefore decided to use a perceptual unit of light 1—interval—light 2, times for which were identical with those for all other conditions, but with varying longer time intervals between each unit. Instead of the metronome, an arrangement of cams, driven by a constant speed electric motor was used to actuate electrical contactors. In this way, it was possible to produce time intervals of 1·5 seconds, 3 seconds, and 6 seconds, between each perceptual unit of light pairs. As the electrical contactors were moved by the cams, a slight metallic rhythmic sound was emitted, but considerably less audible than the metronome beat, and to some extent masked by the noise of the electric motor.

The subjects were shown the lights at two distances apart and at the two quickest intervals, so that there were four sequences of observation for each subject as shown in Table IV below in which the frequency of movement perceptions is given for the ten subjects at each sequence.

TABLE IV

FREQUENCY OF FOUR CATEGORIES OF MOVEMENT PERCEPTION FOR TEN SUBJECTS UNDER CONDITION 5

Distance apart in cm.	*Interval between pairs, in seconds*	*Vertical curve*	*Horizontal curve*	*Straight*	*Two-light movement*	*Total movement*	*Total no-movement*
9·5	1·5	0	0	3	0	3	7
	3·0	0	2	2	0	4	6
6·5	1·5	3	2	3	0	8	2
	3·0	1	5	3	0	9	1
Total ..		4	9	11	0	24	16

Here, for the first time, we find the horizontal-curve perception occurring with greater frequency than the vertical curve, with also a high percentage (46 per cent.) of the total movement now being perceived as straight. There is therefore a marked and statistically significant reduction in the proportion of vertical-curve movement observed under this condition of discontinuous presentation ($\text{chi}^2 = 22{\cdot}20$, $P = <{\cdot}001$). From these results, it might be reasonable to suppose that the discontinuity of the actual light rhythm has had the effect of making perception of straight movement (whether circularly on the horizontal plane or directly straight across the screen) considerably easier, even though a small proportion (17 per cent.) still see the movement as pendular on the vertical plane. The regular rhythm of the light alternation, whether or not it is accompanied by the auditory rhythm of the metronome, would therefore seem to be yet another factor which influences the kind of movement perception that is likely to occur.

To summarize the results of these five experimental conditions, it seems that several factors are contributing to produce the perception of vertical-curve pendular movement so clearly observed by our subjects under the first condition of the experiment. The factor which Johansson describes as inducing the typical W phenomenon of curved motion on the horizontal plane, that of harmonic variation of light intensity in anti-phase, is seen to be only one of several factors combining to produce the vertical, pendular movement.

The relative importance of the various conditions in producing or inhibiting vertical-curve perception is perhaps most clearly shown in the histograms of Figure 3, where each histogram represents the percentage of the total number of movement observations which falls into each of the three main types of movement category, the vertical-curve, the horizontal-curve, and the straight.

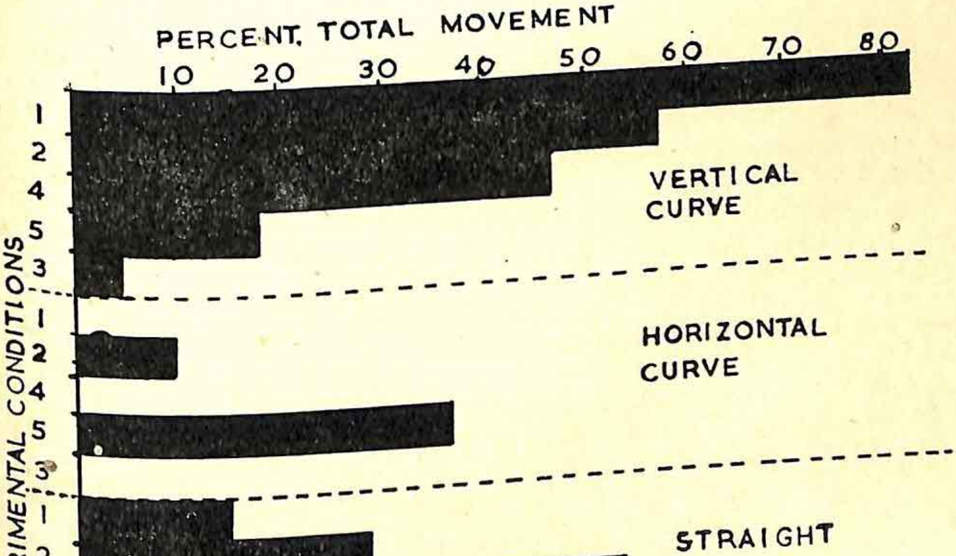

FIG. 3.—Histograms showing the percentage of total movement observations falling in the three main categories for experimental conditions 1–5.

The histograms illustrate clearly that vertical-curve movement is seen most frequently under Condition 1, where the lights are circular, the light-sources are flash-light bulbs, the visual rhythm is continuous and is reinforced by the auditory rhythm, of the metronome. Vertical-curve movement is seen less frequently when the metronome is inaudible, and the amplitude of the perceived curvature appears to be less pronounced, as in Condition 2. Where neon lights are used (Condition 4), the percentage of vertical-curve drops to 45, with a corresponding increase of straight movement to 55. Thus straight movement is seen rather more easily, and vertical curve movement somewhat less easily, where the lights come on instantaneously, even though continuous auditory and visual rhythm are present. Where the continuous rhythm is interrupted by a pause between pairs of lights (Condition 5), vertical-curve movement perception drops considerably, and instead a high proportion of horizontal curve and straight movement is seen (total per cent. for both categories is 83). This seems to indicate that one of the more important factors in the perception of pendular movement as observed under Condition 1 is the continuity of the rhythm. Further experimental variations on this aspect of the problem are now being undertaken. Finally, the squaring of the lights themselves (Condition 3) has a most pronounced effect in straightening the movement perception, so that only 4½ per cent. of the total movement perceptions in this group are vertically curved.

In so far as we have proceeded with this investigation, we can say that the following factors, in probable order of importance, seem to contribute to the pronounced vertical-curve, pendulum-like movement perceptions reported under Condition 1:—

(1) The circular shape of the lights.
(3) The continuity of the rhythmic visual presentation.
(3) The gradient of brightening and dimming of the flashlight bulb filaments.
(4) The reinforcements of these three visual factors by the auditory rhythm of the metronome.

Discussion

We have seen from the results of these five experimental conditions that the original pendular phenomenon of Condition 1 is one of some complexity. Our first hypothesis was that the pendular track was related to the presence of the background auditory rhythm, it being tentatively considered possible that the immediate process of perceiving apparent movement was in some way being brought into relation with memory traces of swinging clock-pendulums evoked by this auditory rhythm.

Our results, however, have shown this to be only a minor background factor, although it is interesting to note that only under this condition (Condition 2) and under Condition 5 when there is also no metronome present, is the phenomenon of curved movement in a horizontal plane observed.

It seems, then, that a more important factor is the visual rhythm of repeated presentation, for, where this continuity is interrupted, in Condition 5, a far higher proportion of straight or horizontal-curve movement is seen. This would seem to indicate that the rhythmic visual presentation may be providing some sort of implicit background or setting in relation to which the subject perceives a unity in the visual stimuli. Very rarely, however, is this background explicitly adduced. At this stage in the investigation it is not possible to say whether experience of clock-pendulums or other like models is a primary factor influencing the immediate perception or whether, alternatively, the subjects' references to such models are merely *rationalizations* introduced to account for a phenomenon in the visual field which is occurring independently in the way that Ekman (*op. cit.*) interprets the W phenomenon.

The complexity of the factors involved is also shown by the fact that, when the light-shape is altered to square, the effect is sufficiently strong to prevent the other factors of visual-auditory rhythm and of the warming-cooling gradient having any influence upon the figural perception which remains consistently that of straight movement. It may be that the top and bottom edges of the square lights, lying, as they do, parallel to the main axis of the display, exert a dominating directional influence on the perception. If so, the effect of this factor can be investigated by varying the angle between the edge of the square lights and the horizontal. On the other hand, the shape factor may be operating in some other way which we should be able to determine by observing the effects of different polygonal shapes upon the movement perception.

With regard to the relation of our findings to those of Johansson, we have previously noted the main differences between the stimulus conditions of his experiments and those of our own. With both lights varying in antiphase, the two perceptions of apparent movement, one at either window, towards and away from the observer, are in some way organized by him into a unitary perception of the apparent movement of a single light source from one window to the other. In our own results, the reduction of curved movement observed under the neon condition (Condition 4) suggests that the distorted harmonic component in the intensity-gradient of our flashlamp bulbs may be a contributory factor to the phenomenon of curved movement, so that to this extent the perception of the W and the pendulum phenomenon may involve some common process. It would seem reasonable to suppose that if, as a further stage in the investigation, we increase the duration of this harmonic variation, we should get a proportionate increase of horizontal-curve (i.e. W) perception.

It still remains for us to account for the fact that interruption of the visual rhythm (Condition 5) has a straightening effect upon the movement perception, or, otherwise expressed, that this condition more easily allows of movement perception on the horizontal plane. Furthermore, our results so far suggest that the longer the interval between pairs of stimuli, the greater the proportion of straight-movement perception that is reported. A provisional hypothesis, then, for further experiment is that, within the limits provided by our special conditions of display, the more discrete or temporarily segregated is one pair of stimuli from the succeeding pair, the less likely is the direction of perceived movement to be influenced away from the horizontal.

As to the relation of our present experimental conditions to those said to be necessary for optimum movement perception, it is only possible to state at present that the spatial and temporal conditions would seem to be just outside the limit for optimum movement quoted by Koffka (1935, p. 292) from Korte's original work. Koffka does not, however, give any data for the brightness factor, so that we cannot be certain at present on this relationship. Johansson (p. 158) also says of the W phenomenon that: "Only when the stroboscopic motion turns into succession does the W phenomenon begin to appear. This indicates that the (physiological) possibility of perceiving succession is a necessary condition for the occurrence of the W phenomenon." He makes this statement on the basis of judgments of two trained observers from whom he derived lower and upper critical temporal limits of W.

Although, therefore, this point must be made clearer in further experiments, it remains possible that the pendulum phenomenon will be found to occur mainly within that region described by Wertheimer (quoted by Boring, 1942, 3. 595) as that of partial and pure movement which lies midway between movement-optimum and succession. This seems to be a region where the stimulus field is relatively unstructured, so that extraneous influences and changes in setting might be expected to have greater effect (Koffka, 1922).

The whole problem of explanation for the original and quite striking pendulum effect of Condition 1 is seen to be one of the relative influences of a complex of stimulus conditions. Other experimental variations, some of which have been indicated, need investigation before we know more precisely the conditions for its occurrence and non-occurrence. For example, we are now engaged in investigating the effect of altering the axis of display from horizontal to vertical. Tentatively, we may conclude that the original phenomenon is due to a strong completion tendency brought about mainly by the conditions of the visual display, but influenced secondarily by the subject's interpretation of what he sees in relation to certain models with which he is familiar and which are acting usually as implicit backgrounds for his immediate perception.

We would like to acknowledge the co-operation of the medical, research and administrative staff of Barrow Hospital in acting as subjects for this experiment. We would also like to thank Professor G. C. Drew for his interest and encouragement of the work, and Mr. J. White, of the Bristol Mental Hospitals' Engineer's Department, for constructing the apparatus.

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ON THE TUNNEL EFFECT

BY

LUKE BURKE, O.F.M.

(From the Psychological Laboratory, Louvain)

The experiments described in this paper consist of presenting observers with two similar objects in successive movement in the same plane but of which the end of the first and the beginning of the second are hidden by a screen (tunnel). Under proper experimental conditions the impression received is one of continuous and uniform movement by a single object passing behind the screen.

The hidden phase of this movement assumes all of the characteristics of true, visible movement for its entire extent, and any lengthening of the duration of the invisible phase gives the impression of a momentary halt behind the screen at a definite point. A modification of the position or of the relative orientation of the visible portions of the trajectory influences the apparent form of its invisible portion. This form can be made to assume the aspects of complicated curves which are sketched and described by different observers with remarkable consistency.

All these phenomena are dependent upon the objctive conditions: the speed of the objects while they are visible, the length of the tunnel, the duration of the invisible phase, the relative position of the visible portions of the trajectory; and they vary systematically with these conditions. As one or other of these conditions is varied, the impression of continuity may give way to one of a simple succession of independent movements.

It is thought that the absence of sensory qualities justifies the use of the term "amodal data" to describe the way in which the hidden movement phase makes itself known to the observer. These "amodal data" form the bridge between the modal phases and become an integral part of the total sensory experience. These properties are determined by the nature of the complex system of excitations in the same way as those of the modal phases and one can thus consider this *amodal* aspect of the combined experience as a truly perceptual phenomenon.

Les expériences décrites dans cet article consistaient à présenter aux observateurs deux mouvements successifs (d'objets semblables) se produisant dans le prolongement l'un de l'autre, mais dont la fin du premier et le début du second étaient cachés par un écran (Tunnel). Dans certaines conditions on peut avoir néanmoins l'impression qu'un objet se meut de façon continue et uniforme et passe simplement derrière le tunnel.

La phase "cachée" de ce mouvement présente tous les caractères d'un mouvement visible sur toute son étendue. Il semble aussi "réel" que celui-ci. Il a une vitesse déterminée, puisque le mouvement d'ensemble paraît uniforme. Un allongement de la durée de la phase invisible (intervalle entrée-sortie) ne provoque pas, en général, un ralentissement apparent du mouvement "caché," mais donne l'impression d'un arrêt momentané qui semble se produire derrière l'écran, à un endroit déterminé. Une modification de la position ou de l'orientation relative des parties "visibles" de la trajectoire influence la forme apparente de sa partie invisible. Cette forme peut éventuellement présenter l'aspect de courbes compliquées qui sont décrites at dessinées d'une façon très semblable par différents observateurs.

Tous ces phénomènes sont strictement dépendants des conditions objectives de l'expérience: vitesse des mobiles dans les phases visibles, longueur du tunnel, durée de la phase invisible, position relative des parties visibles de la trajectoire, et varient systématiquement en fonction de ces conditions. La continuité peut être supprimée et faire place à l'impression d'une simple succession de mouvements indépendants, en agissant sur l'une ou l'autre de ces variables.

L'absence de qualités sensorielles justifie l'emploi du terme de "donné amodal" pour désigner la façon dont la phase "cachée" du mouvement est présente à l'observateur. Ce donné "amodal" fait le pont entre les phases "modales" et se trouve intégré comme partie constitutive dans l'expérience sensorielle globale. Ses propriétés sont déterminées par la nature du système complexe d'excitations au même titre que celles des phases "modales," et l'on peut donc considérer cet aspect "amodal" de l'expérience d'ensemble comme un phénomène d'ordre réellement perceptif.

LET us suppose a cyclist has to pass behind some stationary object in the centre of the street in which we are standing. He moves towards the object, disappears from view and a moment later reappears at the other side. In this everyday fact we have an example of what is known in psychology as the "Tunnel Effect" (Wertheimer, 1912; Sampaio, 1943; Michotte, 1946, 1950; Knops, 1947).

But the fact gives rise to an intriguing problem in the psychology of perception. Every observer is convinced that the cyclist has been *continuously* in motion. Is this merely a matter of belief or of knowledge based on past experience or is the continuity of the movement actually "seen" by the observer? To help in answering this seemingly absurd question is the object of the present paper.

Some years ago in the laboratory of Louvain, A. C. Sampaio carried out researches on phenomenal permanence using the tunnel effect. A few of these experiments were designed to study the identity of the object entering and the one leaving the tunnel, and in this connection she found that the responses were rather inconsistent except for rapid movements and short tunnels. However, when she instructed the observers to judge only the continuity or discontinuity of the movement her findings were very instructive. It was this success that prompted us to extend the research and to study systematically the conditions which give rise to the continuity of the movement, and then to examine the phenomena which occur when these optimum conditions are modified. As will be seen, this led to very interesting and quite unexpected results.

The procedure itself is rather simple. The observer is seated in front of a fairly large white or greyish screen. A narrow horizontal slit is made in this screen, and at a given moment the observer sees an object moving along the slit. A part of the slit, and hence of the movement, is hidden by the "tunnel" which is placed immediately in front of the slit. The experimental conditions can be changed in different ways. Thus, we can vary the length of the tunnel and the length of the trajectory of the moving stimulus, the movement in its speed or its direction, the time between the entry and the exit of the object from the tunnel and, finally, the object which leaves the tunnel may be different from the one which enters.

In actual fact, as we shall see in a moment, the object does not cross behind the tunnel, as there are really two objects, the second of which can be so controlled as to leave after or even before the first has entered the tunnel. There is first a stimulation phase in which an object is moving towards the entry end of the tunnel, then a period of no stimulation (the tunnel) and finally another stimulation phase in which the object moves from the exit end of the tunnel. The movement phases we shall call KSP (kinematic stimulation phase) and the period of no stimulation the EE (Entry-exit interval) which means the time between total disappearance at the entry and the beginning of reappearance at the exit. The EE has a positive value when the exit succeeds the entry; it is zero in the case of simultaneous entry and exit; it can be negative when the exit procedes the entry. As a point of comparison we take into account the time it would take if the object really crossed behind the tunnel at the speed at which it enters. This value we shall call the UCD (uniform crossing duration).

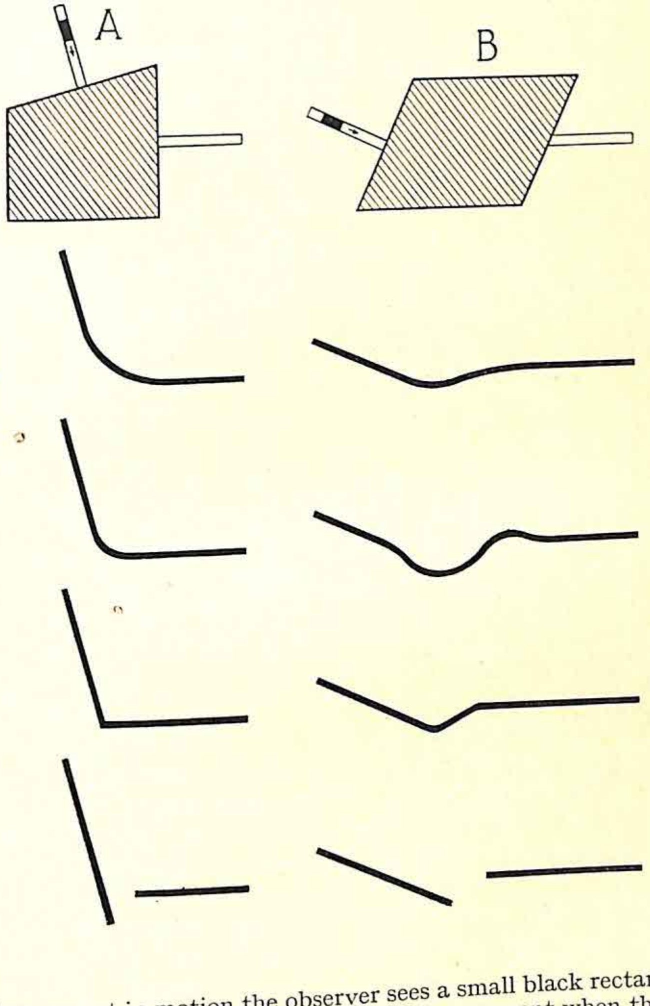
Apparatus

The method of discs and the method of projection adopted by Michotte (1946, pp. 24-38) in his researches on causation are used throughout the present study.

Method of Discs

Two concentric discs of white cardboard are placed on the same rotating spindle, the smaller, B (19cm in diameter) being immediately in front of the larger, A (50cm). On each is drawn a segment of a spiral 8 to 10mm wide, and the discs can be so displaced in relation to each other that the segments form a single continuous spiral, or they can be separated to the extent of any desired angle (see Figure 1). The double disc rotates behind a slit 5mm wide, the length of which can be varied by means of little "wings" of cardboard at each extremity. In the centre of the slit there is another piece of white or coloured cardboard (the tunnel) and the central point of this tunnel coincides with the joining point of the two discs (Figure 1 S).

FIGURE 1



When the discs are set in motion the observer sees a small black rectangle (10 × 5mm) appearing at the left hand side of the slit. (This is the moment when the segment of the larger disc reaches the slit.) The object moves towards the tunnel. (This appearance of movement is brought about by the "spiral." An arc of a circle would produce merely a stationary object.) The object goes behind the tunnel. After an interval of time, which can be regulated in thousandths of a second (ms), an object that is similar in every way to the one which entered, emerges at the exit end and moves at the same speed. (This second stimulation phase corresponds to the segment of the smaller disc.)

The speed of the objects in the KSP depends upon two factors, namely, the rotation speed of the disc and the "fall" of the spiral, the "fall" being the length in centimeters by which the spiral approaches the centre by degrees of angle. The EE can be regulated by changing the angular distance separating the two segments for a given rotation speed of the disc.

Method of Projection

Two projectors are so arranged as to revolve on their own axes at the same speed but independently of each other. At the start of the experiment they are both stationary, but the extent of their movements during the experiment can be controlled by means of little buffers attached to each. Each projector throws an image of a small circle or rectangle of light on to a dimly lighted ground glass screen, at the other side of which is seated the observer. In the middle of the glass screen and on the observer's side there is a piece of black cardboard (the tunnel).

At a given moment the first projector (A) is set in motion and the observer sees the object (A) moving from the right hand side of the screen towards the tunnel and eventually disappearing behind it. By means of a contact pendulum the movement of projector A can command the movement of the second projector (B) and consequently the duration of the EE. At the start of the experiment object B lies stationary behind the exit end of the tunnel (left hand side) ready to move at the desired time.

In all the experiments which follow, the method of discs is used except where it is especially mentioned that we are using the method of projection. In all cases the vision is binocular and the observers are seated at a distance of 1·50m from the apparatus. The KSP lengths are 40mm at each side of the tunnel unless otherwise stated. The instructions to the observers are the same throughout: "Fixate the central point of the tunnel (a small mark at that point aided the observers) and report your impressions of the movement you will see."

Continuity

Before entering the details of our systematic experiments it is important to state that, despite the presence of the tunnel, an absolutely compelling impression of continuous and uniform movement can be produced. Thus, if we take a tunnel of 40mm with EE of ± 15ms and have the object move at a speed of 600mm/s, every observer from the very first glance sees a continuous and uniform movement. In addition, if this is compared with a case of continuous and uniform movement without a tunnel there is no difference between the impressions from the kinematic viewpoint. Besides, all the observers agree that the movement behind the tunnel is as "real" as the movement seen during the stimulation phases. Hence the movement behind the tunnel is real, continuous and uniform.

It is possible to find out the EE values which give similar impressions for other speeds and for other tunnels. But, as we shall see later, the longer the tunnel the less perfect is the impression of continuity, and eventually we reach a length where it is not possible to see continuous movement. It seems, therefore, that there are varying degrees of continuity, not only in the sense of tending towards discontinuity, but also in the pregnance and purity of the impression itself.

This can be shown very well if we take a tunnel of 100mm with EE of 100ms and a speed of 300mm/s. The impression of continuity is not quite so "satisfying" as in our first example. In fact, it often happens that the observers do not see continuity immediately with these conditions, but usually after a few presentations the impression becomes more definite and remains stable from that time on. This seems to imply that with a tunnel of 100mm we are near the limit for continuity in our experimental set-up, and that practice has some effect when the conditions are not optimum. In any case, in systematic experimentation the observers are bound to acquire practice and therefore its influence cannot be excluded. For this reason, in all the experiments that follow in which the method of discs is used the observers see the movement several times successively at every observation. They feel they can define their impressions more satisfactorily and they react more spontaneously to this procedure than to the single presentation method.

Discontinuity

As the EE is increased we would expect that the continuity of the movement would be maintained but that it would cease to be uniform due to a slowing behind the tunnel. But such is far from the case. Instead there is a gradual evolution from continuity to discontinuity in which there are some remarkable and quite unexpected impressions.

Thus, if the EE is increased slightly above the optimum for continuity the observers generally see "a movement with a hitch," that is to say, the object moves behind the tunnel at the same speed but at a certain moment the uniformity is somehow upset. Most of the observers spontaneously indicated the place on the tunnel behind which this interference takes place.

If the EE is increased still further we get a different impression called, curiously enough, "one movement in two phases" (Michotte, 1946, p. 91). The observers used a variety of phrases for this impression, such as the one just quoted or "immediate succession of movements," "two movements with no time or space interval" and so on, all showing that a certain unity of the whole process is maintained in spite of the momentary discontinuity. Again, as with the impression of hitch, the movement does not undergo any slackening.

Finally, when the EE is sufficiently large we reach the stage of discontinuity, that is to say, the entry movement seems to stop and the exit movement seems to start somewhere behind the tunnel. If the tunnel is not too large nor the EE too long, the exit movement begins at the same spot where the entry movement stops. With larger tunnels there appears to be a spatial gap between the two movements so that they are quite distinct and their only relation is that of succession. With these larger tunnels the observers mention also that the entry movement seems "to die out" gradually, and there is thus no phenomenal stop in the movement. In these cases the observers have the impression of a slowing in the movement but without any clear location of the point where it ceases.

As in the case of a movement with a hitch, so also with the impression of a stop or of one movement in two phases many observers spontaneously indicated the point on the tunnel behind which they "saw" these events. When doing so none of them ever mentioned until it was brought to their attention that such events were completely hidden from their view. In this connection it is of interest to make a direct comparison between these various impressions of discontinuity and the impressions of an object whose movement is continuously seen, but objectively stopped for a longer or shorter time. The impressions are astoundingly similar, which seems to show that the phenomenal stopping of the movement (its own time limit) is a specific impression which is not necessarily bound to the fact that the object as such is seen in a stationary position at a given moment.

At the very start of our systematic experiments the observers said they could more easily describe their impressions in graphic form than in words. We encouraged them to do so, and it is interesting to note how accurately these non-verbal responses describe the events. Continuity is represented by a single straight line; a movement with a hitch by a straight line with a small quiver about the middle of it; two phases by two lines joining to make a very wide angle; and a stop by two separate straight lines.

That these impressions are quite specific can be verified by a simple test. Let us take a tunnel of 60mm and have the objects move at a speed of 300mm/s with an EE of about 500ms. Let the exit end be completely covered when the apparatus is set in motion. All we see is an object in movement and disappearing behind the tunnel; the movement "dies out" as with the long tunnels. If we reverse the procedure and cover the entry side we see simply an object appearing in movement from behind the tunnel. And now if we uncover both the entry and the exit the impression is of a sudden stop in the movement. This impression brings something new and this something new takes place in the hidden phase of the movement.

INFLUENCE OF THE LENGTH OF THE TUNNEL

Since our principal aim in this research is the apparent continuity of the movement with tunnel effect, our first task is to determine how the length of the tunnel influences the unity of the movement.

Experiment 1. Four different sizes of tunnels are used, 25, 40, 60 and 100mm. The dimensions of the object are 10 × 5 mm and the speed is 600mm/s. The EE intervals proceed by stages of 35m going from − 50ms to about + 700ms and are presented in a random order for each observer. However, approaching zero we shortened the stages of the EE to 17/18ms.

Experiment 2. The speed is reduced to 300mm/s. The tunnels are 25, 60 and 100mm.

In both experiments the method of discs is employed. Four trained observers took part in experiment 1 and five untrained in experiment 2. There were five sessions for each observer, and at a sitting he was shown one tunnel with all the EE intervals and then another until all four tunnels were presented. Thus for every EE interval an observer made five observations. The order of presenting the tunnels was changed each time for each observer. In experiment 2 the EE intervals did not go to negative quantities as we were interested here primarily in the upper limits of continuity.

TABLE I

THE EE VALUES IN MS CORRESPONDING TO THE IMPRESSIONS. SP. 600 MM/S

Tunnel	Observers	*Continuity*				*Hitch*			*Two phases*			*Stop*	
		1st cases	*Average*	*Last 100%*	*Last cases*	*1st cases*	*Average*	*Last cases*	*1st cases*	*Average*	*Last cases*	*1st cases*	*1st 100%*
25 mm UCD: 42 ms	M	−31	30 (32)	56	91	73	114 (10)	161	161	217 (18)	301	231	336
	K	−14	29 (29)	56	91	73	115 (13)	196	161	226 (20)	301	301	336
	Y	−14	36 (30)	56	91	73	138 (17)	196	196	244 (13)	301	301	336
	B	−41	22 (33)	56	91	73	127 (15)	196	161	238 (16)	336	266	371
	AVERAGE ..	−22	29	56	91	73	124	187	170	231	310	275	345
40 mm UCD: 67 ms	M	−21	37 (28)	67	84	84	129 (19)	189	154	252 (15)	329	364	364
	K	− 3	39 (28)	49	101	67	125 (19)	189	189	260 (21)	364	329	399
	Y	− 3	56 (32)	84	154	101	154 (16)	224	224	278 (17)	364	294	399
	B	−21	40 (31)	84	101	101	147 (19)	224	189	272 (16)	329	329	364
	AVERAGE ..	−13	43	71	110	88	139	207	189	266	347	329	382
60 mm UCD: 100 ms	M	− 5	56 (34)	82	117	100	159 (20)	222	187	293 (21)	397	327	432
	K	− 5	46 (32)	64	100	82	149 (24)	222	222	289 (20)	362	362	397
	Y	+12	77 (30)	64	152	82	173 (21)	257	222	297 (15)	362	362	397
	B	− 5	65 (37)	100	134	117	175 (14)	222	257	318 (19)	327	327	432
	AVERAGE ..	− 1	61	78	126	95	164	231	222	299	380	345	414
100 mm UCD: 167 ms	M	+16	83 (29)	104	138	121	180 (27)	226	226	307 (19)	401	296	471
	K	+34	99 (37)	121	191	138	205 (24)	296	226	336 (22)	471	401	506
	Y	+51	110 (27)	121	191	138	209 (30)	296	296	354 (17)	436	401	471
	B	+16	95 (38)	138	174	156	223 (30)	331	261	366 (17)	471	401	506
	AVERAGE ..	+27	97	121	175	138	204	287	252	346	445	375	489

First and last cases mean the lowest and highest EE for which there was at least one case of that impression.
The figures in brackets indicate the number of cases of that impression for a particular observer.
100% is the EE for which an observer has the same impression in all five presentations.

Continuity

From Tables I-IV it is seen that the EE values for continuity increase with the length of the tunnel. This is only what could be expected as soon as there is phenomenal movement since the duration of this movement depends on the length of the trajectory. (The analysis of variance of the averages for continuity of Table I gave $F > 33$ for 3 and 12 d.f. beyond the 1 per cent. point of significance. A similar analysis shows that the difference in EE for continuity between any two tunnels is significant. Using the same figures of Table I the t test beyond the 5 per cent. point gave $x_j - x_k > 12{\cdot}7$.)

TABLE II

DISTRIBUTION OF THE FREQUENCIES OF THE IMPRESSIONS. SP. 600 MM/S

Tunnel 25mm					*Tunnel 40mm*					*Tunnel 60mm*					*Tunnel 100mm*				
EE	*Cont.* %	*Hitch* %	*2 Ph.* %	*Stop* %	EE	*Cont.* %	*Hitch* %	*2 Ph.* %	*Stop* %	EE	*Cont.* %	*Hitch* %	*2 Ph.* %	*Stop* %	EE	*Cont.* %	*Hitch* %	*2 Ph.* %	*Stop* %
−49ms	0	—	—	—	−38ms	0	—	—	—	−23ms	0	—	—	—	− 1ms	0	—	—	—
−31	20	—	—	—	−21	10	—	—	—	− 5	50	—	—	—	+16	10	—	—	—
−14	95	—	—	—	− 3	70	—	—	—	+12	65	—	—	—	34	45	—	—	—
+ 3	100	—	—	—	+14	100	—	—	—	30	95	—	—	—	51	75	—	—	—
21	100	—	—	—	31	100	—	—	—	47	100	—	—	—	69	95	—	—	—
39	100	—	—	—	49	100	—	—	—	64	100	—	—	—	86	100	—	—	—
56	100	—	—	—	67	90	10	—	—	82	90	10	—	—	104	100	—	—	—
73	70	30	—	—	84	85	15	—	—	100	85	15	—	—	121	95	5	—	—
91	35	65	—	—	101	30	70	—	—	117	50	50	—	—	138	65	35	—	—
126	0	100	—	—	119	5	95	—	—	134	25	75	—	—	156	35	65	—	—
161	—	55	45	—	154	5	90	5	—	152	5	95	—	—	174	20	80	—	—
196	—	25	75	—	189	—	70	30	—	187	—	95	5	—	191	15	85	—	—
231	—	—	95	5	224	—	15	85	—	222	—	70	30	—	208	—	100	—	—
266	—	—	95	5	259	—	—	90	10	257	—	10	90	—	226	—	80	20	—
301	—	—	25	75	294	—	—	70	30	292	—	—	100	—	261	—	55	45	—
336	—	—	5	95	329	—	—	55	45	327	—	—	90	10	296	—	50	50	—
371	—	—	—	100	364	—	—	10	90	362	—	—	45	55	331	—	5	90	5
					399	—	—	—	100	397	—	—	10	90	366	—	—	85	15
										432	—	—	—	100	401	—	—	55	45
															436	—	—	20	80
															471	—	—	10	90
															506	—	—	—	100
546				100	539				100	532				100	647				100

This table is formed by pooling the results of all the observers.
The first EE for all four tunnels has 0% continuity. At this level all the impressions were of two parallel movements.

It can also be seen from the tables that the impressions of continuity for both speeds extend over quite a range of EE values, even to negative values for the smaller tunnels. The question naturally arises—is the apparent speed the same in all? To try to test this delicate point we gave two very practised observers the task of direct comparison.

Experiment 3. One case is a tunnel of 100mm with an EE of 100ms and a speed of 300mm/s and the other is the same except that the EE is changed to zero.

One of the observers knew nothing of the conditions nor the aim of the experiment; the other knew there was a difference but did not know which was which. We could change quickly the conditions from one to the other and the observers were asked to look away from the apparatus while the adjustments were being made. The instruction was to compare the two cases from the point of view of the speed. They made 10 observations each.

In every observation there was the impression of continuous movement but the zero case was invariably judged the faster. Moreover, it was the *whole movement*, visible as well as invisible, and not merely a part of it that appeared the faster. It was judged to be a continuous movement but with a progressive acceleration all through its trajectory. There is no doubt that the apparent speed of the movement varies according to the EE value and is different in the different cases of continuity.

We have already remarked that the shorter the tunnel the better the impression of continuity. This is evident when we consider that it implies the perfect integration of the two KSP and the hidden movement. Therefore, any factor unfavourable to this integration, such as increase in the spatial separation of the KSP (longer tunnels), will render more difficult the impression of continuity. Is this obvious fact shown in our numerical results (Tables I-IV)?

TABLE III

THE EE VALUES IN MS CORRESPONDING TO THE IMPRESSIONS. SP. 300 MM/S

		Continuity		*Hitch*			*Two phases*			*Stop*	
Tunnel	*Observers*	*Last 100%*	*Last cases*	*1st cases*	*Average*	*Last cases*	*1st cases*	*Average*	*Last cases*	*1st cases*	*1st 100%*
25 mm UCD: 84 ms	H	112	112	147	179 (13)	217	217	264 (11)	287	322	322
	P	112	112	147	189 (17)	252	252	279 (9)	322	322	357
	B	42	77	77	140 (15)	182	182	239 (13)	287	287	332
	Z	77	112	112	150 (11)	182	157	235 (13)	287	252	322
	E	7	77	42	145 (19)	217	217	252 (10)	287	252	322
	AVERAGE ..	72	98	105	161	210	203	254	294	287	329
60 mm UCD: 200 ms	H	157	192	192	258 (18)	332	332	370 (14)	402	437	437
	P	157	157	192	249 (21)	332	332	357 (10)	402	402	437
	B	122	157	157	235 (19)	297	297	344 (12)	402	367	437
	Z	87	157	122	194 (14)	227	262	326 (22)	437	367	472
	E	192	192	227	262 (15)	297	332	357 (11)	402	402	437
	AVERAGE ..	143	171	178	240	297	311	351	409	395	444
100 mm UCD: 333 ms	H	220	325	255	330 (22)	395	430	482 (20)	535	570	570
	P	220	255	255	332 (23)	430	395	468 (16)	535	535	570
	B	185	290	220	318 (20)	430	360	418 (12)	500	465	535
	Z	185	255	220	317 (21)	395	395	440 (15)	500	465	535
	E	220	220	255	341 (29)	465	430	465 (8)	500	500	535
	AVERAGE ..	206	269	241	328	423	402	455	514	507	549

TABLE IV

DISTRIBUTION OF THE FREQUENCIES OF THE IMPRESSIONS. SP. 300 MM/S

Tunnel: 25mm. UCD: 84ms				
EE	*Cont.*	*Hitch*	*2 Ph.*	*Stop*
	%	%	%	%
7ms	100	—	—	—
42	96	4	—	—
77	92	8	—	—
112	48	52	—	—
147	—	96	4	—
182	—	92	8	—
217	—	44	56	—
252	—	8	84	8
287	—	—	72	28
322	—	—	4	96
357	—	—	—	100
462	—	—	—	100

Tunnel: 60mm. UCD: 200ms				
EE	*Cont.*	*Hitch*	*2 Ph.*	*Stop*
	%	%	%	%
52ms	100	—	—	—
87	100	—	—	—
122	96	4	—	—
157	84	16	—	—
192	24	76	—	—
227	—	100	—	—
262	—	80	20	—
297	—	72	28	—
332	—	8	92	—
367	—	—	92	8
402	—	—	40	60
437	—	—	4	96
472	—	—	—	100
579	—	—	—	100

Tunnel: 100mm. UCD: 333ms				
EE	*Cont.*	*Hitch*	*2 Ph.*	*Stop*
	%	%	%	%
115ms	100	—	—	—
150	100	—	—	—
185	100	—	—	—
220	88	12	—	—
255	28	72	—	—
290	8	92	—	—
325	4	96	—	—
360	—	88	12	—
395	—	72	28	—
430	—	24	76	—
465	—	4	72	24
500	—	—	64	36
535	—	—	24	76
570	—	—	—	100
640	—	—	—	100

The mere comparison of the absolute values of EE can give us little information on this point since such durations increase with the size of the tunnel. A better method would be to compare for each tunnel the EE for continuity and the EE which gives complete segregation (stop). It is evident that if certain experimental conditions are favourable to integration, the relative increase in EE to give complete segregation for these conditions must be stronger than where the conditions are less favourable. This conclusion is verified in the numerical results if we establish the ratio between the last 100 per cent. continuity and the first 100 per cent. stop.

Naturally we do not attribute a definitive value to these ratios since the number of observations is small. Nevertheless the tendency is quite clear, the ratios decrease with the increase in the length of the tunnel. In other words, the increase in EE to arrive at discontinuity is greater in the cases where the spatial separation of the KSP is least and consequently where the integration is stronger.

TABLE V

RATIO BETWEEN LAST 100 PER CENT. CONTINUITY AND FIRST 100 PER CENT. STOP

Tunnel	25*mm*	40*mm*	60*mm*	100*mm*
600mm/s	6·2	5·4	5·3	4·0
300mm/s	4·6	—	3·1	2·7

Discontinuity

Here also it is clear from Tables I-IV that the EE's for the different impressions of discontinuity increase with the length of the tunnel. But there arises again, as for continuity, the question of the apparent speed of these various impressions of discontinuity. To solve the problem we used once more the method of direct comparison.

Experiment 4. One case is a tunnel of 100mm with an EE of 100ms and a speed of 300mm/s and the other is the same except that the EE is raised to 300ms.

Several observations were made by two trained observers who were asked to compare the two cases from the point of view of the apparent speed. With the EE at 100ms there was always the impression of continuous and uniform movement, but at 300ms it was a movement with a hitch and there was the impression of a general slackening of the *whole movement*. We repeated the experiment with comparisons of continuity and two phases, and continuity and a stop. There was always the same difference—the continuous and uniform movement was judged to be faster than any of the other cases. This is another indication that the whole movement in all its phases has the EE as one of the factors of the apparent speed.

We have already remarked that the EE's for the various impressions increase with the length of the tunnel. But if we examine Tables I-IV we find, except for the case of clear discontinuity (stop), that the time (EE) increases more slowly than the space (the tunnel). The same results were found by Sampaio (unpublished results). But they were also found in the case of stroboscopic motion (Koffka, 1935, p. 293). This might indicate that we have here a general rule for the perception of movement of different lengths which could be related to the underestimation and overestimation occurring in serial experiments. In our experiments this would mean an underestimation of the length of the tunnel and/or an overestimation of the EE.

However, another possible explanation is the segregating power of the tunnel. When we increase the size of the tunnel this means that the two KSP are further separated in space and consequently their unification becomes increasingly difficult. To compensate for this increase there would be a relative decrease in the value of the EE.

Very long tunnels

So far, the longest tunnel used was 100mm. Even with a speed of 300mm/s the impression of continuity was quite clear. It would be interesting to discover what would happen if much longer tunnels were used. The method of discs is not convenient for this purpose so we used the method of projection.

Experiment 5. The lengths of the tunnel are 60, 200, 300, 400, 500 and 700mm with 40mm for the KSP in all cases. The size of the object is 5 × 7mm and the speed is 360mm/s. The EE's proceed by stages of from 40 to 50ms, going from −50ms up to two or three seconds. The conditions are therefore as close as we could manage to those of experiments 1 and 2 with the method of discs.

There were two reasons which made us include in this series the short tunnel of 60mm. Firstly, it would afford us a point of comparison with the results obtained in similar conditions with the method of discs. Secondly, it could be expected that the observers would have some difficulty with such long tunnels, but if a simple case, such as 60mm, were shown first it would help the observers to describe their impressions. Three practised observers performed the experiment once for each tunnel, the results being so clear as not to demand any further repetition.

With the tunnel of 60mm there was an exact parallel with what the same observers reported in the method of discs. But there was quite a different story when we came to the long tunnels. Except on one occasion for the tunnel of 200mm, and that was a doubtful impression, there were no cases of continuous movement. The observers did sometimes mention "continuity in time" by which they meant that the entry and exit movement formed a kind of rhythmical unity, but they always expressly excluded any impression of a crossing of the tunnel. The object moved behind the tunnel as in the simple screen effect, then the movement seemed to "die out," and after an interval of time another movement began near the exit end of the tunnel. Between these two there was what the observers called curiously but aptly "an empty space." It appeared that this "empty space" had nothing to do with the EE, although the observers considered that the space was longer as the tunnel increased in length. No matter what intervals were used there was never the impression of a sudden stop or of anything resembling two phases or hitch. Would a faster speed help to bring about a unity? This was the aim of the next experiment.

Experiment 6. The conditions are the same as in experiment 5 except that the speed is increased to 630mm/s.

The results were just as clear as in the previous case—with tunnels larger than 200mm in our experimental conditions the continuity of the movement was never mentioned. We can conclude that there is an upper spatial limit for continuity with the tunnel effect. As we shall see, this limit varies with the speed of the movement. There may be other factors as well, such as the extent of the KSP and the size of the objects, but since we have maintained these two constant in our researches we are not in a position to state their influence.

INFLUENCE OF THE SPEED

We have already remarked, a high speed is very favourable to the continuity of the movement, and yet, as we have seen, there is a range of speeds within which the impression is quite satisfactory. If a decidedly slower speed is used, how will this affect the impression?

Experiment 7. The experimental conditions are the same as in experiment 1 and 2, the only difference being a reduction of the speed to 62mm/s.

From the very start it was evident that the impressions were quite different from those obtained with a faster speed. Thus, for a very short EE with the tunnels of 60 and 100mm the observers said that the object left the tunnel "too early." (The EE's in this experiment proceeded by stages of 161ms, which was rather large, but necessarily so if we were to have any appreciable difference in the stages). If smaller intervals are used with the tunnel 25 mm, especially when near the zero point, it will be found that the impression "too early" occurs here also.

This is the first time in our researches that we have recorded such an impression, and it is rather remarkable that the observers never speak of an acceleration of the movement, which is what happened for small EE's with faster speeds. It seems to imply an anticipation or appreciation of the duration of the movement behind the tunnel at entry speed and therefore involves practice or learning. But it is quite clear that "too early" denies a uniform crossing of the tunnel. The very expression itself betrays a preoccupation with time rather than with movement.

The observers recorded "continuity" in this experiment, but undoubtedly the impression was not the same as with the faster speeds. The observers—they were the same as those of experiment 2—spontaneously compared the two impressions and all agreed that a faster speed gave the impression of continuous movement behind the tunnel, whereas here it was mostly a matter that the "object comes out on time." It is to be noted also that the observers seemed to feel ill at ease with this experiment and took longer to report their impressions. An exception might be made for the small tunnel (25mm) where for certain EE's the observers had an impression of continuous movement which was comparable to that of the faster speed. However, it seems that, even in the case of continuity, the time factor is more dominant than the kinematic factor.

TABLE VI

The EE Values in ms Corresponding to the Impressions. Sp. 62 mm/s

Tunnel	*Observers*	*Continuity*		*Delay*			*Too late*	
		Last 100%	*Last cases*	*1st cases*	*Average*	*Last cases*	*1st cases*	*First 100%*
25 mm UCD: 403ms	H	190ms	351ms	351ms	683ms	1156ms	834ms	1317ms
	P	190	512	351	630	834	834	995
	B	351	512	512	657	834	834	995
	Z	351	512	512	780	995	995	1156
	E	351	512	512	754	995	995	1156
	Average ..	287ms	480ms	448ms	701ms	963ms	898ms	1124ms
UCD: 968ms	H	891ms	1052ms	1052ms	1250ms	1535ms	1374ms	1696ms
	P	730 (80 %)	891	730	1085	1374	1213	1535
	B	730	891	891	1081	1213	1213	1374
	Z	569	891	730	1068	1374	1213	1535
	E	730	891	891	1131	1374	1374	1535
	Average ..	730ms	923ms	922ms	1123ms	1374ms	1277ms	1535ms
100mm UCD: 1613ms	H	1350ms	1511ms	1350ms	1765ms	2155ms	1994ms	2316ms
	P	1189 (80%)	1511	1189	1650	1994	1672	2155
	B	1350 (80%)	1672	1028	1570	1994	1672	2155
	Z	867	1350	1028	1525	1833	1833	1994
	E	1028	1511	1189	1590	1994	1672	2155
	Average ..	1147ms	1511ms	1157ms	1620ms	1930ms	1769ms	2155ms

With a fast speed, an increase in the EE brought about the very distinctive impressions of hitch, two phases and stop. With the slow speed there arose instead the vague impressions of "delay" and "too late," expressions which show again a preoccupation with time. At the beginning it was not quite clear how "delay" differed from "too late," but the distinction was made by the observers and so it was maintained in our tables of results. When they were questioned on the matter it appeared that "delay" meant that "the object does not become completely lost" as in the case "too late." This would mean that by "too late" the object was no longer "present" to the observers.

The numerical results of Tables VI and VII show, as with the higher speeds, that the EE's corresponding to the different categories of responses, increase with the length of the tunnel, but again, in most cases the increase is not proportionate. Furthermore, if we apply the criterion of the strength of integration by establishing the ratios of first 100 per cent. too late with the last 100 per cent. continuity, we find again that the integration is stronger with the short tunnels (see Table VIII). However, the very considerable length of EE which gives "continuity" for this slow speed seems to confirm what has already been said by the observers. The EE's, especially for the tunnels 60 and 100mm, are so far beyond anything we have yet found that we are led to conclude that such "continuity" is a matter of time anticipation and not a movement impression as such.

TABLE VII

DISTRIBUTION OF THE FREQUENCIES OF THE IMPRESSIONS. SP. 62MM/S

Tunnel: 25mm. UCD: 403ms				
EE	*Too early*	*Cont.*	*Delay*	*Too late*
	%	%	%	%
29ms	—	100	—	—
190	—	100	—	—
351	—	76	24	—
512	—	16	84	—
673	—	—	100	—
834	—	—	52	48
995	—	—	4	96
1156	—	—	4	96
1317	—	—	—	100
1800				100

Tunnel: 60mm. UCD: 968ms				
EE	*Too early*	*Cont.*	*Delay*	*Too late*
	%	%	%	%
247ms	76	24	—	—
408	64	36	—	—
569	28	72	—	—
730	—	92	8	—
891	—	60	40	—
1052	—	8	92	—
1213	—	—	84	16
1374	—	—	40	60
1535	—	—	4	96
1696	—	—	—	100
2340				100

Tunnel: 100mm. UCD: 1613ms				
EE	*Too early*	*Cont.*	*Delay*	*Too late*
	%	%	%	%
384ms	80	20	—	—
545	72	28	—	—
706	64	36	—	—
867	40	60	—	—
1028	8	84	8	—
1189	—	84	16	—
1350	—	64	36	—
1511	—	36	64	—
1672	—	4	84	12
1833	—	—	56	44
1994	—	—	28	72
2155	—	—	8	92
2316	—	—	—	100
2960				100

On several occasions we have remarked that a high speed is favourable to the continuity of the movement, and this point now receives a further confirmation. If we refer back to Table V we find that a higher ratio is required for segregation with a speed of 600mm/s than for 300mm/s. In other words, the integration is stronger with a faster speed. Now if we refer to a similar table (Table VIII) for the speed of 62mm/s we find that the ratios are still lower, thus showing that segregation has already been considerably aided by the use of such a slow speed.

TABLE VIII

RATIO LAST 100 PER CENT. CONTINUITY/FIRST 100 PER CENT. TOO LATE

Speed 62mm/s	*Ratio contin./too late*
25mm	3·9
60	2·1
100	1·9

Conclusion

If we consider all the data so far collected, it seems that with high speeds and relatively small tunnels the kinematic structure predominates. This is especially evident where the uniformity of the movement is no longer perfect, for then the impressions show alterations of the movement itself, such as hitch, two phases and stop. Besides, there is usually the possibility of locating these events, thus emphasising their spatial characteristics.

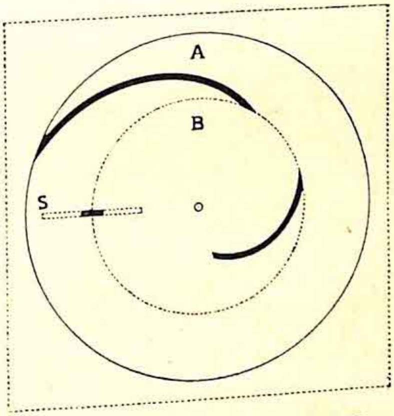
On the contrary, when a very slow speed is used the kinematic structure is scarcely evident and the temporal properties take a prominent place. This becomes clear when we consider the extremely long EE intervals for "continuity" with a slow speed. Even with the most favourable spatial conditions for integration (tunnel 25 mm) the very first cases of segregation (delay) are at a level of half a second at the minimum. Such high intervals seem incompatible with the phenomenal presence of a movement crossing behind the tunnel. This phenomenal presence seems to be limited by the spatial separation of the KSP (very long tunnels) and the duration of their temporal separation (long EE's resulting from a slow speed).

Such a conclusion is confirmed by the numerical results. If we compare the EE's for the first 100 per cent. stop for the high speeds (from 329 to 549ms) with the last cases of continuity for the slow speed and a tunnel of 25mm (namely, 480ms) it could be taken as a hypothesis that an interval of half a second would be about the extreme upper limit of time for genuine kinematic continuity in our experimental conditions.

INFLUENCE OF THE RELATIVE POSITION OF ENTRY AND EXIT TRAJECTORIES

Some casual observations had shown that if the trajectories of the two KSP formed an angle instead of being a continuation of each other horizontally, certain extremely remarkable phenomena could be produced. In these preliminary trials it was discovered that with the appropriate time conditions the movement could appear just as continuous as in our previous experiments but the phase behind the tunnel took on a special shape. The results were so impressive that we decided to undertake a more systematic study and some special experiments with the method of discs were designed for the purpose. The conditions were similar to those already used except that now the entry slit sloped towards the exit and the joining point of the two could be covered by various lengths of tunnel. We devised three such cases, the length of the tunnel as well as the angle formed by the slits being somewhat different in each.

FIGURE 2



Experiment 8. (*a*) The length of the slits is 80mm and they make an angle of 105°. The tunnel (the distance between the entry and exit) is 90mm and the slits are perpendicular to the sides of the tunnel. (See Figure 2A). slits are perpendicular to the sides of the tunnel. (See Figure 2A).

(*b*) The conditions are similar to the preceding except that the angle is 115° and the tunnel is 75mm long.

(*c*) The angle is also 155° and the tunnel 75mm, but due to its position and shape the exit slit seems to begin at a higher level than the ending of the entry slit as in the Poggendorff illusion. (See Figure 2B).

In all three cases the size of the objects is 5 × 15mm and the speed is 300mm/s. There are four EE intervals the longest of which is about 600ms and the shortest just about avoids the possibility of seeing the two objects on the slits at the same time.

As observers we had 18 University students the majority of whom had never taken part in psychological experiments. Each observer performed all three experiments in one sitting which was rather short since we used only four EE intervals for each of the three cases. We thought it wiser to increase the number of observers as much as we could rather than repeat the experiments many times with the same observers as there would be a danger of stereotyped responses. They were seated at a distance of 1·50m from the apparatus and looked at the screen binocularly. The instructions were carefully considered and this was the form addressed to each: "Fixate the central point of the tunnel. Describe in words what you see when the apparatus is set in motion and on this sheet of paper try to draw as accurately as you can what you have observed."

As could have been expected the observers showed different aptitudes for drawing as for keenness of observation, and yet from the two sources, verbal and non-verbal, we get results that show a high degree of regularity.

All observers had the clear and compelling impression of continuous movement when the EE was suitably short, but the movement behind the tunnel did not follow a straight line from the entry to the exit but instead took the form of a curve. The first two cases differed little except that the curve was steeper in the first than in the second, due to the smaller angle. But in the third case the curve took a complicated form in which the entry movement curved below and then upwards to the exit. In all three cases the curves became more flattened with a further shortening of the EE. For most of the observers the curves began *before* the entry, and in the exit slit there was often a wavy movement just after the object had left the tunnel. This would seem to indicate that the influence of the EE is not limited to the hidden phase but extends to the visible phases as well. (We had found something similar in experiment 3 where a shortening of the EE gave an acceleration of the *whole* movement.) In Table IX this impression is described as "curve."

TABLE IX

Distribution of Frequencies of the Impressions

	EE	*Curve*	*Angle*	2 *Movts*
		%	%	%
(*a*)	90ms	100	—	—
	130	39	61	—
	300	—	84	16
	650	—	22	78
(*b*)	75ms	100	—	—
	180	50	50	—
	250	—	89	11
	600	—	22	78
(*c*)	75ms	100	—	—
	180	44	56	—
	250	—	89	11
	600	—	16	84

As the EE became longer the movement, although continuous, took on a shape tending to be angular, the angle being formed by the entry and exit movements. In Table IX this impression is referred to as "angle" and it seemed to resemble very closely the impression of one movement in two phases recorded in previous experiments. It is noteworthy that none of the observers ever mention "hitch" an impression so characteristic of our first experiments. Instead, a lengthening of the EE shows the trajectory as more curved and tending to become angular.

Finally, when the EE was very large, there was the impression of a stop or of two movements. The numerical data are to be found in Table IX.

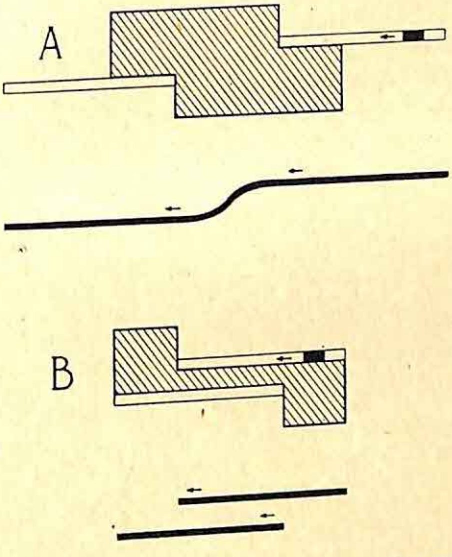
We made a comparison of the drawings submitted by the observers and in Figure 2 we reproduce four typical shapes corresponding to different lengths of EE (the shortest above). The general outlines of these shapes can be found in the drawings of practically all the observers. It is a very remarkable fact that almost all omitted to draw the tunnel or to make any distinguishing mark between the various phases of the movement. Moreover, after the experiments were ended many of the observers were curious to discover how it was possible to have such a variety of movements. They were really surprised to find out that it was done by merely changing the EE. Even then, on viewing the experiments again they all said that they could not describe their impressions otherwise than they had done.

In addition to the set observations here recorded, we showed the experiments to a large number of visitors to the laboratory, among whom were psychologists from different countries, and the results followed the same pattern as those recorded in Figure 2.

This rather unexpected result led us to devise two further experiments this time with the entry and exit horizontal to each other but at different levels.

Experiment 9. The entry slit is 80mm long; and at a level of 10mm below but at a distance of 50mm is the exit slit, also 80mm long. (See Figure 3A). The size of the objects is 7 × 5mm and the speed is 360mm/s. The EE intervals are 50, 100 and 200ms.

FIGURE 3



Experiment 10. The conditions are the same as in the preceding except that the entry and exit slits instead of being 50mm apart now overlap each other to the distance of 50mm. (See Figure 3B.)

The method of projection was used in both experiments. About 50 observers took part, two thirds of them being entirely unpractised. They were seated at a distance of 1·50m from the apparatus and were instructed to fixate a point at the centre of the tunnel. They could report their impressions verbally or by drawing the course of the movement, or they could use both methods if they wished. About half of them saw first experiment 9 and immediately afterwards experiment 10. The order was reversed for the others.

The very first trial with experiment 9 was made with Professor Michotte who had suggested the experiment. In accordance with theoretical considerations he really expected to see the hidden phase as a straight line from entry to exit. He was extremely surprised to find that this was not so, but instead there was a very compelling impression, from the first presentation, of an S-shaped trajectory behind the tunnel. In our systematic experiments all our observers without exception reported likewise. There was a continuous movement whose trajectory was shaped like a flattened S, and the S became more flattened as the EE decreased. (See Figure 3A for a sample of the drawings). With experiment 10 all the observers reported on the contrary two movements parallel to each other but unconnected. (See Figure 3B). There was never the impression of continuous movement which on the face of it was rather surprising. It would seem just as easy to see now a Z-shaped trajectory as an S-shaped in experiment 9.

These three experiments show in a very striking way how the spatial pattern of the slits influences the shape of the movement behind the tunnel. This movement adapts itself to the relative position of the entry and exit slits. However, if this spatial pattern demands a backward movement then there is disintegration and no continuity. It would seem that continuity is possible as long as the exit trajectory takes more or less the same direction as the entry, or at least that it does not require a reversal of the movement.

INFLUENCE OF A CHANGE IN THE OBJECT AT THE EXIT

One final problem suggested itself to us in this study of continuous movement with tunnel effect. If the object leaving the tunnel were different in some way from the one which entered, this would seem at first sight to be an unfavourable factor for the impression of continuous movement. To test this point we devised some experiments in which the colour, size and shape of the objects would be different but the length of the tunnel as well as the size of the KSP and the speed of the movement would remain unchanged.

Experiment 11. Object A (the one entering the tunnel) is identical to object B (the one leaving the tunnel), both being light circles of 4·25cm in diameter.

Experiment 12. Objects A and B are the same size as in experiment 11 but A is white and B is a bright rose colour, the two being more or less of the same brightness.

Experiment 13. Objects A and B are two light circles of the same brightness and colour but of different dimensions. A is 4.25cm and B is 5·60cm in diameter.

Experiment 14. Object A is the original white light circle of 4·25cm in diameter, but object B is a white light square whose side is 4·25cm.

The method of projection was used in all four experiments. The tunnel was 12cm long with 10cm of KSP at each side. The speed of the movement was 360mm/s and the UCD was 333ms.

Five observers, all of whom had some practice in this type of experiment performed each experiment five times. There were about 20 EE intervals going by stages of from 30 to 40ms, beginning at – 50ms and rising to ± 600ms, and these intervals were presented in a random order for each observer. The order of the experiments was as above, each observer finishing experiment 11 before starting experiment 12. The instruction was simply to remark if *the movement appeared continuous or not*. After the experiments were completed the observers were asked as a supplementary question whether the changes in the object made any difference to their impressions of continuity. The following were their answers to this question.

Observer *P* did not find that the changes disturbed the impressions of movement. However, when there was clear discontinuity there was the impression of two distinct objects especially when they changed their shape or their size. On the contrary when there was clear continuity there was no doubt that it was one and the same object which passed behind the tunnel, but in doing so changed its shape, size or colour.

Observer *B* noticed the changes only at the beginning of each experiment. After a short time the differences became unimportant and the experiment proceeded as if there were no changes at all. This observer could notice the changes by concentrating on them, but found it easier to consider the experiments as of one object which changed behind the tunnel.

Observer *E* considered the changes of colour as the most disturbing factor. Continuity meant there was continuity of the movement but this observer had the constant impression of "two objects acting as carriers of one and the same movement."

Observer *R* also found the change of colour the most disturbing factor. (It is worth remarking that this is not reflected in the results of *E* or of *R*.) For this observer the impression of continuity was strongest in the case of identity.

Observer *K* said that continuity always implied the identity of the objects no matter what changes took place. The object did not really change its colour but there was rather a change in the lighting conditions. The impressions of continuity were weakest with a change of shape.

TABLE X

The EE Values in ms Corresponding to the Impressions

Last 100% *Continuity—*

Observers	*Identity*	*Colour*	*Size*	*Shape*
P	255ms	325ms	255ms	325ms
B	295	220	255	255
E	220	295	220	255
R	325	255	255	255
K	325	255	295	295
First 100 *per cent. Discontinuity*				
P	400	455	430	455
B	400	365	430	455
E	400	400	430	455
R	430	365	400	400
K	400	325	430	430

The numerical results (of which a summary is given in Table X) as well as the descriptive data of the observers clearly indicate that the changes in the object at the exit do not really disturb the continuity of the movement, and that for the most part the experiments proceed as if there were no changes at all. Whatever differences there are in the results for the four experiments seem to be due to chance.

General Conclusions

In all the experiments described in this paper the observers are shown two short movements between which there is a spatial gap (the tunnel). But the observers say they "see" the movement in the "hidden" phase ! Moreover, without the slightest difficulty or hesitation they all make drawings of what they have "seen," and in these drawings it is rare to find any indication of a tunnel or any mark by which to distinguish the "hidden" from the overt phases. Practically all the observers trace the paths of the movements as if there were no tunnels.

In order to have more precise information on the characteristics of the "hidden" phase, we showed the experiments to several people highly skilled in observation in psychology or in the natural sciences. All agree that the "hidden" phase can not be described in terms of sensory qualities. They see neither the colour nor the form of the object during its course behind the tunnel. After the event they may succeed in imaging the trajectory as a clear or a dark line, but at the moment of perception there is nothing of this kind. In fact, the aspect of the "hidden" movement is unaltered whether we use dark objects and bright tunnels (method of discs) or bright objects, coloured or not, with a dark tunnel (method of projection). In both cases the movement is without any discernible brightness or colour at the moment of perception.

Besides, when the observers have the impression of a stop in the movement behind the tunnel, this does not mean that they see the object motionless. And yet, when asked to compare it directly with a movement continuously seen in which the object stops for a moment, all observers say "the two impressions are identical !"

All the foregoing facts point in the one direction. They lead us to affirm without any doubt, it seems to us, that there can be a phenomenal "presence" of a movement which is without any discernible sensory quality (like the "reine Bewegung" of Wertheimer).

To designate this fact, Professor Michotte has suggested the term "*amodal* data of sensory experience," thus indicating the absence of any sensory modality (Helmholtz), that these "amodal" data are given in a sensory experience and that it is dependent on the system of stimulation of the senses. This will help to understand the apparent paradox implied in the very words quoted at the outset of this paper—"seeing a hidden movement."

The possibility of "amodal" data in our experience can have a considerable bearing on other problems, notably on the perception of volume as has been demonstrated by Michotte (Michotte and Burke, 1951). However, our own experiments do not extend to this matter so that we cannot examine it here. What we have shown is the existence of the "amodal" phase of movement with tunnel effect and the conditions under which it occurs. A further study would be to relate our findings to analogous facts whose laws are well known such as the Phi phenomenon or certain configurations studied by Gestalt Psychology. It is hoped to make this the object of another paper to be published shortly. The role of past experience in "amodal" data would also be a very fruitful line of research.

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PHENOMENAL CAUSATION AT A DISTANCE

BY

MARIANO YELA

(*From the Laboratory of Experimental Psychology, Louvain*)

The aim of this research was to study the finding by Michotte, that a moving object *A* can apparently produce movement in a projectile *B* without making contact with it.

The experiments confirm the existence of a causal impression of "pushing at a distance," but they demonstrate that the greater the distance the smaller the chance that this impression will occur.

When it does, in fact, occur, it has the same characteristics, and is associated with the same experimental conditions, as the impression of "pushing with contact."

Temporal continuity must obtain between the stopping of the moving object and the starting of the projectile; this refutes any theory positing that there is a "passage" of movement from one object to the other. The effects of differences in speed of movement, whether absolute or relative, are similar in both instances.

In general, however, it appears that distance slightly accentuates the segregation of the movements and that the temporal and kinematic conditions of integration require to be more favourable in the case of distance, if an impression of pushing is to be given which is as satisfying as that found in the case of direct contact.

The size of the Radius of Action, that is, the extent of the passive phase of the projectile, is of the same order in the two cases.

The results bring out the fundamentally temporal-kinematic nature of the perceptual pattern of pushing. They appear difficult to reconcile with an interpretation based on past experience, but tally with the theory of "Ampliation of Movement" put forward by Michotte. According to this theory the essential point lies in the phenomenal *transitory belonging* to *A* (the moving object) of the movement performed by *B* (the projectile).

Le but de ces recherches était d'étudier le fait décrit par Michotte, qu'un object en mouvement peut provoquer apparemment le mouvement d'un projectile sans entrer en contact avec lui.

Les expériences ont confirmé l'existence d'une impression causale de "lancement à distance," mais elles ont montré que plus la distance est grande et plus faible est la probabilité de présence de cette impression.

Lorsque celle-ci se manifeste, elle a les mêmes caractères, et se montre liée aux mêmes conditions expérimentales que l'impression de lancement avec contact direct.

Il doit y avoir continuité temporelle entre l'arrêt de l'objet moteur et le départ du projectile, ce qui exclut toute théorie faisant intervenir un "passage" du mouvement d'un objet à l'autre. L'influence de la vitesse, absolue ou relative, des mobiles est semblable dans les deux cas.

D'une manière générale cependant, il semble que la distance accentue légèrement la ségrégation des mouvements et que les conditions temporelles et cinétiques d'intégration doivent être plus favorables pour donner une impression aussi satisfaisante que dans le cas du contact direct.

La grandeur du Rayon d'Action, c.à.d. l'étendue du parcours du projectile affectée d'un caractère de passivité est du même ordre dans les deux cas.

Les résultats obtenus font ressortir l'aspect foncièrement temporel-cinétique de la structure perceptive du lancement. Ils paraissent difficilement conciliables avec une interprétation basée sur l'expérience passée, mais s'accordent avec la théorie de "l'Ampliation du mouvement" proposée par Michotte, selon laquelle le point essentiel résiderait dans l'appartenance phénoménale transitoire à l'objet moteur, du mouvement exécuté par le projectile.

I

INTRODUCTION

In his work *La perception de la causalité* (1946), Michotte has shown that certain combinations of the movements of two objects produce impressions which observers commonly describe by words implying mechanical causation as pushing, kicking, throwing and the like. On these facts he developed a theory—the theory of "ampliation"—according to which the responses are the consequence of the structural organization of the perceptual phenomenon itself. Furthermore, he established the fact that such phenomena depend on the kinematic system of stimulation. They are indeed so closely bound to it that every change in the stimulus conditions is followed by systematic changes in the corresponding impression.

Such concomitant variation makes it rather difficult to ascribe the forming of the "causal impression" to the influence of past experience. The same conclusion is supported by other data. There are, for example, the "negative cases," in which certain combinations of events fail to give any causal impression, although we have learned and know perfectly well they are cases of physical causation. There are also the "paradoxical cases," in which, on the contrary, a "causal impression" is obtained within conditions under which we know on the basis of past experience that no physical causation can take place.

Among the latter a very striking example is that of pushing at a distance ("lancement a distance"). Here an object *A* moves towards another object *B*, but it stops before touching *B*. At the moment that *A* stops, *B* starts to move in the same direction. When observers are presented with such a situation they report the impression that "*A* pushes *B* away"—a fact obviously contradictory to our everyday experience.

This phenomenon, only incidentally recorded at the time, seemed to us to deserve further study. Our aim was to study systematically the fact and to examine its theoretical implications.

The Method

We have used the method of discs, devised by Michotte (1946, p. 25 f.). In the main it consists in drawing on a cardboard disc two segments of spiral curves, each of which is preceded and followed by an arc of a circle, and letting the disc turn behind a screen out of which has been cut a horizontal slit 5 mm wide and 200 mm long. Through the slit a horizontal band of the disc is continuously seen. When the disc is revolving the arcs give to the observer the impression of small motionless rectangles (objects *A* and *B*, 5 × 8 mm). But as soon as the spiral segments are seen through the slit, these objects seem to move. The extent and velocity of the movements depend on the characteristics of the spirals as well as on the speed of revolution of the disc.

II

CAUSATION AT A DISTANCE

The Fact

At the beginning of the experiment object *B* is motionless at the middle of the slit. Object *A* appears on the left and moves to the right up to a certain point. *B* begins then to move to the right and after a course of 5 cm disappears behind a wing covering a part of the slit. The time relation between the stopping of *A* and the starting of *B* as well as the distance (*D*) separating the stopping point of *A* and the initial position of *B* can be changed at will.

In our first set of experiments the time interval between the two movements was 15 ms (milliseconds). The velocities were 30 cm/s for *A* and 4·5 cm/s for *B*, the ratio between them being 6·7/1, a ratio very favourable to the impression. The *D*'s used were 0, 10, 30 and 50 mm.

The subjects observed the screen binocularly from a distance of 1·5 m, their eyes being at the level of the slit. There were eight subjects: four members of the Laboratory and four students. None except Michotte and ourselves knew the purpose of the experiment. Each subject was tested alone and was kept ignorant of the results, of his own as well as of the others. We presented each subject with the four *D*'s, one at a time and in random order. The instructions were to look at the screen and report whatever might be seen.

Results. As far as distances 0 and 10 are concerned, the response of all subjects was the same: "*A* pushes *B* away." The impression was reported as equally clear in both cases. It was not so with *D*'s 30 and 50. With these distances some subjects always reported a clear impression of pushing, whereas for others, at the beginning, the impression was ambiguous: sometimes it was pushing, sometimes it was two independent movements. The impression of pushing, however, appeared spontaneously with all subjects after a few trials. It is worth noting that the impression arose more easily when the subject fixated object *A* or a point lying somewhere between *A* and *B*, or again when the event was attended to as a whole. On the contrary the impression easily disappeared when object *B* was fixated. Even though reporting causation in all cases, the observers remarked that the impression in the case of the greater distances was "less satisfying" than in the case of contact.

Thus, causation seemed to be perceived with all *D*'s, but with some qualitative differences. To investigate further these points we proceeded in the following way.

The Frequency of the Response with Increasing Distance

The conditions were the same as before, but a new disc with 90 mm *D* was added to the series. From a group of 250 students belonging to different faculties and not previously connected with experiments of this type, 50 subjects were assigned at random to each *D*. To each student only one distance was presented. He was asked to look at the screen and to report what he saw. The experimenter covered the slit with a screen and after a warning signal he uncovered it letting the subject see a single instance of the two movements until object *B* disappeared. Then he covered the slit again and repeated the operation until the subject gave an answer. As a rule, three or four presentations at the most were required.

When the subject's response clearly stated causation it was recorded as positive. Examples: "The square at the left pushes the other," "it is like kicking at football." When the answer clearly stated two independent movements, it was recorded as negative. Examples: "Two objects moving in succession," "when one rectangle stops, the other moves." For *D*'s 30, 50 and 90, in about a third of the cases, the answers were vague or irrelevant. Examples: "Two objects," "movements," "a square moving around," "black squares," etc. Whenever the answer was not explicit enough with regard to our problem we called the subject's attention to the two moving objects, asking him: "Do you see these two movements as independent of each other or do they seem to you to be related in some way?" If pushing was reported the response was recorded as positive; otherwise, it was taken as negative.

Results. Table I gives the results for the five distances. The results show that the impression of pushing is related to the distance between the two objects at the moment that *B* starts: the greater this distance, the smaller the chance of eliciting from the subject a response of pushing. Nevertheless, we have found again that this response appears, although not always, with all distances employed, so that we may conclude that under our experimental conditions observers actually report causation in spite of the distance.

TABLE I

FREQUENCIES OF CAUSAL RESPONSE WITH INCREASING DISTANCE

Distance, mms	*N*	*Absolute frequency*	*Per cent.*
0	50	50	100
10	50	34	68
30	50	27	54
50	50	22	44
90	50	14	28
	250	147	

The frequencies are significantly different. If (147)(50)/250 is used as the expected frequency of pushing responses for all *D*'s, $\chi^2 = 25{\cdot}2$, which is significant for 4 d.f. beyond the 1 per cent. level. Even if we exclude from the table the case of 0 distance, $\chi^2 = 8{\cdot}7$ for the other four frequencies, also significant for 3 d.f. at the 5 per cent. level.

It must be noted, however, that under certain circumstances the distance case may be more favourable to the causal impression than the case of contact. Essential to the impression is the segregation of objects *A* and *B*, and this depends on visual acuity. When the acuity is sufficiently low in cases of spatial contiguity, one sees simply a continuous movement of one and the same object crossing another which is stationary (Michotte, 1946, p. 44 f.). In the case of distance, however, it seems that the distance itself makes possible the distinction of the objects even for low acuity grades, allowing the subjects to perceive pushing in cases of extreme indirect vision where the impression has altogether disappeared for the case of contiguity.

It might be objected however that, with our technique, the distance is not necessarily perceived as "empty distance." Thus the observers might see the white surface of the disc between the stopping position of *A* and the starting position of *B* as a third solid object lying between them and along which the shock could be transferred. That would be a case of "instrumental causation" already experimentally studied (Michotte, 1951, p. 202) and frequently met in everyday life, e.g. in the game of crocquet. If this objection held, then our experimental situation of "pushing at a distance" would not be contrary to past experience. As a matter of fact, some subjects did see the separating distance as an object, although others maintained it was an empty space.

To clarify this point we devised an experiment in which the perception of the separating space as an "object" was ruled out.

Causation at a Distance in Free Space

We wanted the separating distance to appear as "empty space." This could be obtained by a stereoscopic procedure by which the two objects appeared as two small greyish circles freely floating and moving in tri-dimensional space.

We used the method of anaglyphs as follows. Two rotating projectors were arranged in such a way that when the first (*A*) stopped, the second (*B*) began to move (Michotte, 1946, p. 31 f.). Each projector threw on a ground glass screen two little light circles, one red and the other green (complementaries). The observer, seated at the other side of the screen, looked at them through spectacles suitably fitted with coloured filters and he saw two small greyish circles about 1 cm diameter floating in the air somewhere between him and the screen. These floating images seemed to be located inside a kind of a large box, the front and back of which had been removed. This box was placed there in order to help the location of the circles in the third dimension, and it proved to be very effective in facilitating the fixation and assuring a stable convergence.

The experiment began with object *B* motionless. Then, at a given moment, circle *A* appeared in motion going towards *B* at an apparent velocity of 25 cm/s and stopped at a distance of 10, 35 or 65 mm from *B*. At the moment that *A* stopped, *B* moved away at the apparent speed of 16 cm/s, and after travelling a few centimeters disappeared behind a screen.

The experiment was carried out at three different sittings with five practised subjects. Several foreign psychologists visiting the Laboratory also made observations.

The results are conclusive: although the objects seemed floating in the air, the pushing of *B* by *A* was perfectly clear to all. There is thus not the slightest doubt that such a phenomenon may really occur.

Conclusions of Section II

The fact that there is perception of pushing at a distance is established. The impression, however, has less chance of being reported when the distance increases.

We must proceed now to the study of its properties and conditions, limiting ourselves to the cases in which the causal response was given.

III

THE STIMULUS PATTERN

A good way of doing this study would be to find out whether the responses in the case of pushing at a distance could be shown to depend on the same conditions as in Michotte's experiments of pushing with contact. Also, it was important to see if there were any systematic differences due to the values of the *D*'s.

Now, pushing with contact has been shown to depend on a definite stimulus pattern. It demands a certain temporal continuity of the two movements. It occurs only within a certain range of absolute speeds. And it is facilitated by a certain ratio between the velocities of the objects. Our purpose is to study these factors in the case of distance. Let us consider first the temporal factors.

TIME RELATIONS BETWEEN THE TWO MOVEMENTS

The conditions of the experiments were the standard ones, except that a series of time intervals were introduced between the moment of the stopping of *A* and the moment of the starting of *B*. These intervals were varied at random from 0 to 400 ms by steps of 16·7 ms. Also, we changed the colour of object *B* to red so that the two objects would not appear as a single black one while they remained contiguous in the case of contact. Five subjects—three members of the Laboratory and two new students—were presented with *D*'s 0, 10, 30 and 50. There were five sessions for each subject. In each session the observer made two responses for each time interval, the *D*'s being presented one at a time and in different order at each sitting. At the end, each subject had given 10 responses for each interval and each *D*. The *D*'s were presented to the group in all possible orders (24 permutations). The experiments only began for each subject when he had spontaneously reported causation in all *D*'s for zero time interval.

Before examining the results it is necessary to make a preliminary remark. In his research on the contact case, Michotte (1946, p. 8 f.) had found that when the time interval was progressively increased the subjects responded in three different ways: immediate pushing, pushing with delay, and two independent movements. Pushing with delay was a kind of retarded causation, object *B* appearing as if sticking to object *A* for a brief instant before being pushed away. These three types of impressions were also reported spontaneously by our subjects. In the case of distance there was, of course, no impression of *B* sticking to *A*, but simply of *B* apparently being pushed away after a brief delay.

Results. The results are given in Table II and Figure 1, where the data from all five subjects have been pooled. They give the percentages of each type of response for each time interval and the four *D*'s. The results show there are certain similarities and certain differences between the case of contact and the cases of distance. Let us first consider the similarities.

TABLE II

Distribution of Responses, in Percentages, for each Time Interval (N = 50 per time interval and *D*)

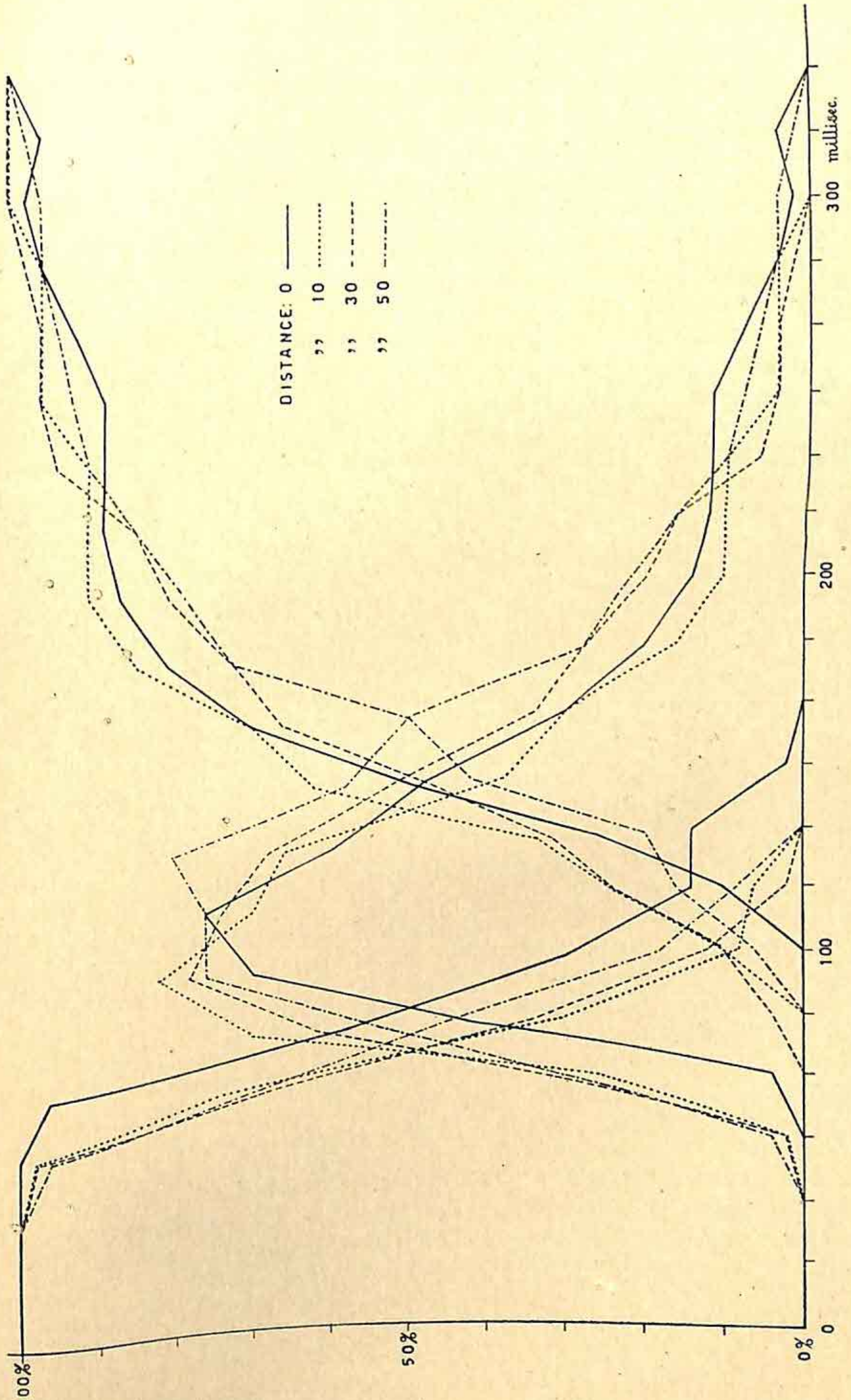
Intervals (*ms*)	*Distances in mm between stopping of A and starting of B*											
	0			10			30			50		
	IP	*PD*	*TM*	*IP*	*PD*	*TM*	*IP*	*PD*	*TM*	*IP*	*PD*	*TM*
0	100	0	0	100	0	0	100	0	0	100	0	0
17	100	0	0	100	0	0	100	0	0	100	0	0
33	100	0	0	100	0	0	100	0	0	100	0	0
50	100	0	0	98	2	0	98	2	0	96	4	0
67	96	4	0	74	26	0	68	32	0	70	30	0
83	58	42	0	30	70	0	34	62	4	46	54	0
100	30	70	10	8	82	10	12	78	10	18	76	6
117	14	76	10	6	70	24	2	74	24	8	76	16
133	14	60	26	0	66	34	0	68	32	0	80	20
150	2	48	50	0	38	62	0	52	48	0	58	42
167	0	30	70	0	30	70	0	34	66	0	50	50
183	0	20	80	0	16	84	0	28	72	0	28	72
200	0	14	86	0	10	90	0	20	80	0	22	78
217	0	12	88	0	10	90	0	16	84	0	16	84
233	0	12	88	0	10	90	0	6	94	0	10	90
250	0	12	88	0	4	96	0	4	96	0	8	92
267	0	8	92	0	4	96	0	4	96	0	6	94
283	0	4	96	0	4	96	0	2	98	0	4	96
300	0	2	98	0	0	100	0	0	100	0	4	96
317	0	4	96	0	0	100	0	0	100	0	2	98
333	0	0	100	0	0	100	0	0	100	0	0	100
350	0	0	100	0	0	100	0	0	100	0	0	100
367	0	0	100	0	0	100	0	0	100	0	0	100
383	0	0	100	0	0	100	0	0	100	0	0	100

IP = Immediate pushing. PD = Pushing with delay. TM = Two movements.

(1) These responses depend systematically on the increase of the time interval for all four *D*'s.

(2) The distribution of relative frequencies of the different responses over the various time intervals presented for all *D*'s, roughly the form of the standard psychometric function, characteristic of discrimination processes around the threshold.

Fig 1



(3) In all *D*'s, 100 per cent. of the responses were of immediate pushing for short intervals—from 0 to 33 ms. With increasing intervals, impressions of pushing with delay appeared and they predominated from 100 to 133 ms. Still larger intervals gave the predominant impression of two movements in all distances until a certain interval—about 300 ms—from which on, 100 per cent. of the impression were of two independent movements.

But if there were similarities there were also differences. In Table II and Figure 1 it will be seen that starting from an interval of 50 ms the percentages of immediate pushing are always slightly greater in the case of contact than in the cases of distance. In the same way the upper time limit for immediate pushing is highest in the case of contact. Also the impressions of pushing with delay and of two movements began to appear in all cases of distance at lower time intervals than in the case of contact. Our results for the case of contiguity can be compared with similar ones mentioned by Michotte (1946, pp. 87–89 and p. 109). It will be remarked, however, that the impression of immediate pushing is maintained in our experiments for larger time intervals than in those researches (Prof. Michotte himself was an observer in both cases). The same thing happens with the impression of pushing with delay. This must be due, undoubtedly, to the fact that we have used a fairly large velocity ratio, which is known to be an integrating factor.

Conclusions. The results as a whole show, therefore, that as far as temporal continuity is concerned, pushing at a distance obeys the same laws as pushing with contact. On the other hand, they show that the two movements are slightly less integrated in the case of distance, since the impression of pushing in that case breaks down with smaller time intervals than in the case of contact. This corresponds to our previous finding that longer distances give fewer causal impressions. Another important fact is that with the impression of pushing, time and space factors, however equally important they may be from a logical or physical point of view, are in no way comparable from the phenomenal viewpoint. Phenomenal time continuity seems to be very important for the impression of pushing. Phenomenal space contiguity is in many respects irrelevant.

All these facts lead to a conclusion of theoretical importance. The causal impression is not due to a propagation of the movement from the moving object to the moved. Propagation would take time, and this time would increase with the distance, as has been clearly shown by extensive experiments on the "tunnel effect" carried out at Louvain by Sampaïo (1943, p. 28), and more recently by Burke (1952). In such experiments a moving object may seem to pass at continuous and uniform speed behind a "tunnel" which covers a part of its trajectory. Here there is apparent propagation of the movement through the "tunnel," i.e. through the separating distance between the two visible parts of the movement. And here again, as the separating distance increases, if the impression of continuity is to be maintained, the time interval between the two visible parts, i.e. the time for propagation, must increase more or less proportionately. In other words, for propagation, the greater the separating distance the greater the time interval. In our case, on the contrary, the time intervals are practically independent of the separating distances. Indeed, they seem to be in opposite relations, that is to say, the greater the distance the shorter the time. So our conclusion is borne out: in pushing at a distance there is no phenomenal propagation of the movement. The impression of pushing is bound to continuity in time, but indifferent to continuity or discontinuity in space. We shall see later how this fact can be understood within the general theory of phenomenal causation.

ABSOLUTE VELOCITY

In the case of contact it has been shown (Michotte, 1946, p. 101) that slowing down the absolute velocities of the two objects weakens the impression of pushing until at a certain low velocity it disappears completely, giving place to the impression of two independent movements. We will try to find out if the same thing holds for pushing at a distance.

We have presented seven subjects (among whom three unpractised) with the four standard *D*'s 0, 10, 30 and 50. The speed ratio was always 6·7/1. The absolute velocities of object *A* were 30, 15, 7·5, 3·9 and 3 cm/s. In all cases as the speed decreased the impression of pushing became weaker and doubtful, and eventually disappeared. However, it was extremely difficult to decide which was the minimum velocity necessary for the impression with the different *D*'s. A new approach seemed necessary if we were to obtain more precise data on this question.

We presented the subjects with two *D*'s which they were to compare directly. One of them, *D* 0, was the standard; the other, *D* 50, the variable. The ratio 6·7/1 was again kept constant. The speed of object *A* for *D* 0 was always 12·4 cm/s, but it varied for *D* 50, being 12·4, 15, 20·6 and 30 cm/s. In another series the constant speed of object *A* was 7·5 cm/s for *D* 0 and the variable ones for *D* 50 were 7·5, 12·4, 15, 20·6 and 30 cm/s. Five practised subjects acted as observers. They were asked whether the impression of pushing with *D* 50 was similar, better or worse than with *D* 0.

Results. Here again the results show that the impression of pushing was very much the same with the different *D*'s at all speeds. The case of contact, though, seemed in general "more satisfying." This is particularly true when both cases are compared under the same conditions and at slow speeds. With increasing speed, in the case of distance, the discrimination begins to be difficult. Responses of equality, although mostly doubtful, become dominant when the speed of the distance case is about twice the speed of contact. When it is more than twice, surprisingly enough, the case of distance seems to be the better one.

RELATIVE VELOCITY

Michotte has shown that a decreasing speed ratio was favourable to the causal impression (1946, p. 102 f.). The faster the speed of *A* with respect to the speed of *B*, within certain limits, the stronger the impression of pushing. If this is the case we can predict that all other things being equal, the impression will require greater speed ratios the stronger the segregating factors it has to compensate. Thus, if object *B* tends to be more autonomous in the case of distance, we should expect that a greater speed ratio would be required to give the same impression as in the case of contact. That such is the case was shown by the following experiments.

The conditions were more complicated than in the previous experiment because we wanted to study the influence of the speed ratio and at the same time to see if the absolute speed affects this influence. We prepared, thus, four sets of experiments in which the standard was now *D* 50 and the variable *D* 0. For *D* 50 the ratio was always 6·7/1, but the absolute speed differed from set to set, the velocity of object *A* being respectively 30, 12·4, 7·5 and 3·9 cm/s.

The comparison case *D* 0 presented within each set different speeds combined in such a way that the ratios were 6·7/1, 4/1, 3/1 and 2/1, taking as unity for each set the same speed for object *B* as in the corresponding standard. For example, in the first set the standard velocities for *D* 50 were 30 and 4·5 cm/s, and the velocities for the comparison case *D* 0 were 30 and 4·5; 18 and 4·5; 13·5 and 4·5; and 9 and 4·5 cm/s.

Results. The five subjects of the previous experiment took part in this one also. For the fastest speed and when both cases, contact and distance, have the same ratio 6·7/1, the impressions are comparable, although the contact case is somewhat better.

The impressions become more nearly equal when the speed ratio for the case of distance is about twice as great as the ratio for contact. The distance case gives even a better impression when its ratio is higher than twice that of contact. It can be seen that these results agree with those obtained before with increasing velocity. But a decrease in the absolute speed is decidedly favourable to the case of contact. The case of distance is so affected by very slow speeds that even with a ratio more than three times that of contact the causal impression remains poorer. For the lowest speed the impression of pushing at a distance becomes very vague or disappears altogether for most subjects. At this low speed even the impression for contact becomes at times vague and doubtful.

Another remark must be added in regard to the speed ratio. In the case of contact it had been found that when the speed ratio was less than unity, i.e. when the speed of *A* was slower than that of *B*, the impression changes from pushing to "release" (Michotte, 1946, p. 103). We presented a great number of subjects—trained and untrained—with the standard *D*'s 0, 10, 30 and 50, at different increasing speed ratios with greater speeds for *B* than for *A*. In all cases, contact and distance, all subjects eventually reported the release effect. It is probable that because of the disintegrating influence of the distance, impressions of "release" would occur a little earlier than in the case of spatial contiguity, i.e. for velocity ratios that would still favour "pushing" in the latter case. But we have not made systematic observations on this point.

Conclusions of Section III

To summarize, the results from this section—referring only to cases in which "causal impressions" were reported by the observers—point to the same conclusions, namely:—

(1) The differences found between the case of contact and those of distance were generally very slight. No systematic difference was detected by our methods between the various *D*'s (note that the smallest was 10 and the largest 50 mm).

(2) When the experimental conditions for both cases were similar, except for the distance, the causal impressions were somewhat "more satisfying" in the case of contact—a fact which may betray a stronger integration in this case.

(3) But, strange as it might seem, the causal impressions for the distance case can, under certain conditions (greater velocities or ratios), be better than for contact.

In a few words, it seems that both cases—contiguity and distance—depend in the same way on the same objective conditions. The spatial gap has proved to be practically ineffective.

IV

THE RADIUS OF ACTION

The most important feature of the causal experience is perhaps what Michotte (1946, p. 49 f.) has called the radius of action, RA. In his experiments with the case of contact the subjects pointed out that object *B* seemed to be pushed away by *A* only up to a certain distance from its point of departure. Beyond that distance *B* seemed just to be travelling along entirely independent from *A*.

Thus, the apparent passivity of the movement of *B*, obviously an essential feature of the causal impression, vanishes at a certain point, from which on there occurs a change in the phenomenal appearance of that movement. The extent of the passive phase was called the radius of action.

When, on the contrary, there is no causal impression and, therefore, the movements are seen as two successive but independent events, *B* appears to be active from the start, and its movement seems to be its own all the way through. In such a case it would be meaningless to try to establish from which point on it changes from passivity to activity: there will be no RA.

The radius of action discussed here refers, of course, to causation. The notion may be extended to other cases. Michotte (1946, p. 54 f.) has used it with regard to the "going away" of an object from another. When two objects stand together and then one of them moves away, it seems to go "away from" the other, but only up to a certain point—the "rayon d'action de l'écartement." Beyond this point the moving object seems just to move on without any reference to the stationary one. We have tried to find out whether the same thing happens in cases like ours, when at the start there is a distance between the objects. The results were negative. It seems that the "effet écartement" of Michotte happens only in cases of actual contact, where the objects form at the beginning a strong static unit.

Moreover, Michotte has shown that in the case of contact it was possible to measure such a RA. Then, if this proves to be possible also in our experiments with the case of distance, it could be rightly considered as a kind of touchstone for the presence of a true causal impression and, therefore, as a check on the purely descriptive verbal responses of the observers that were given in Section II of this paper.

In order to test this point we made a new series of experiments. Eight subjects (four practised and four unpractised without any previous connexion with these experiments) were presented with *D*'s 0, 10, 30 and 50, at the constant velocity of 30 cm/s for *A* and 4·5 cm/s for *B*. The total length of the trajectory of object *B* was 10 cm. When all subjects had reported the pushing, we reduced the trajectory of *B* to a few mm, increasing it then progressively in successive trials. The observers were asked whether they detected any change in the character of the movement of *B*. They all spontaneously noticed without any difficulty the evolution from passivity to activity. Then they were told that a series of experiments was to be made in order to measure the extent of the passive course of *B*. When the task was proposed all untrained subjects were sceptical about it. After a few trials, however, all reported that the task was possible and not too difficult. We then proceeded to the systematic measurement of the RA using the method of limits.

Each subject made 20 series of observations for each of the four *D*'s in five sittings, i.e., four series for *D* in each sitting, alternating ascending and descending series. The order of presentation of the *D*'s was so arranged as to compensate for any influence it might exert on the results (all possible combinations). The length of the trajectory of *B* could be changed by shifting a small cardboard wing along the slit behind which the objects were moving. We measured the distance between the left side of object *B* (its rear end toward *A*) and the left end of the wing. The measurement was thus the distance covered by *B* from the start until it totally disappeared. In each series this length was varied from 8 mm (the length of the object) to 63 mm by steps of 5 mm. The observers were instructed to say "Yes" when they had the impression *B* was pushed along its entire visible path, and "No" when they had the impression *B* went too far for the push. The RA in each series was the length at which the subject first changed the response from "Yes" to "No" or vice versa.

Results. Table III gives the mean length of the RA for each subject and each *D*, each one of these values being thus the average of twenty measures. Table IV gives the corresponding variances.

The first thing to be noticed is that the phenomenon was quite clear to all subjects in all cases, and that they could determine the length as easily in the cases of distance as in that of contact. This subjective feeling is reflected in the quantitative data. It can, in fact, be seen that the variances of each individual are of the same order in the case of the four *D*'s. Bartlett's test for homogeneity of variance shows that these values are not significantly different for the first six subjects. Only in the case of two subjects, G and R, are there some four significant differences, but completely unrelated to the distance, as can be seen in Table IV. In general, thus, the differences among the variabilities of each subject for the four *D*'s are not significant. This fact would seem to suggest that the RA is, quantitatively as well as qualitatively, of the same character and equally precise for all *D*'s.

TABLE III

RADIUS OF ACTION IN MM

Distance	*Subjects*								*Average*
	M	*B*	*Be*	*L*	*Y*	*K*	*R*	*G*	
0	30	32	32	37	29	30	39	41	34
10	31	33	29	35	30	27	39	40	33
30	31	34	30	37	32	28	36	41	34
50	34	34	30	33	31	27	37	39	33

TABLE IV

VARIANCE OF THE RADIUS OF ACTION

Distance	*Subjects*								*Group variance*
	M	*B*	*Be*	*L*	*Y*	*K*	*R*	*G*	
0	13·19	13·19	7·75	39·00	6·19	8·19	5·69	22·50	21·20
10	11·19	8·19	17·75	27·19	7·19	8·19	5·69	39·00	22·58
30	13·69	6·00	10·69	38·19	11·50	7·75	1·19	16·19	18·87
50	10·69	11·19	8·19	43·69	11·19	7·25	2·25	58·50	14·75

The high reliability of the RA for each subject is shown in the fact that the standard errors of the various mean RA are about 1 mm, in most cases less, only with subjects L and G they are sometimes as great as 1·5 mm. This high degree of reliability may seem to be rather astonishing for measurements of such imponderables as those in question here, and one may wonder if they are not due to some experimental error (stereotyped reactions for a given length of trajectory, too large steps, etc.). But this does not seem at all to be the case. The same variability has been found by Michotte, using the adjustment method (1946, p. 51), and moreover, new experiments that we have carried out one year later, and which will soon be published gave the same results. These new researches were concerned with the influence of various conditions, as for instance, the speed on the size of the RA. The standard errors were again of the same order, although the spatial values of the RA substantially varied from one series to the other, which left little chance for stereotyped responses.

It remains to be seen whether the distance, while not affecting the qualitative character and the reliability of the RA, had some effect upon its magnitude.

Table III shows also the average RA for each *D*, and Table IV the corresponding variances. There is no significant difference between these variances. Similarly, it was found that there were no significant differences between the values of the RA for the four *D*'s (Table V).

TABLE V

ANALYSIS OF VARIANCE OF THE RA'S FOR THE FOUR *D*'S

Source of variation	*Sum of squares*	*d.f.*	*Mean square*	*F*
Between experimental situations . .	4·5526	3	1·5175	
Between subjects	492·2636	7	70·3234	29·8
Residual (error)	49·5568	21	2·3598	
Total	546·3730	31		

Finally, the length of the RA, in the case of contact, has been shown to depend upon the velocity of the process (Michotte, 1946, p. 50 f.). It becomes systematically greater with increasing speed. We have found that that is also true with the cases of distance. An increase in the absolute velocities produced always in all subjects an obvious increase in the length of the RA for all *D*'s. The differences were so great and clear that no statistical analysis seemed necessary.

Conclusions of Section IV

We feel justified in concluding from the above results that, so far as the RA is concerned, there are no significant differences between pushing with contact and pushing at a distance.

On the other hand, the individual differences are rather considerable and without doubt significant (F = 29·7 for 7 and 21 d.f., cf. Table V). It is, of course, difficult in cases like this to ascribe them with certainty to any particular reason without first making extensive researches on many subjects. One thing, however, deserves mentioning in this respect. All observers pointed out that there is no clear-cut limit between the passive and the active character of the movement of *B*. This appears first clearly passive, then changes gradually, becomes ambiguous, until finally it seems clearly active. It is thus likely that different subjects adopt spontaneously certain criteria which may remain very stable for each of them, but which may be very different from one to another. Possibly this could eventually explain the individual differences.

Thus, the great difference between the values of the RA which we have found and those mentioned by Michotte (1946, p. 51) for about the same speed of *B*—9·6 mm for a speed of 4 cm/s, may be purely a matter of individual differences. But it is also possible that it is due to the different size of the objects or to the method used: adjustment in that case, method of limits here. Furthermore, it is by no means excluded that the two methods could favour the use of different criteria.

V

GENERAL CONCLUSIONS

All the results described in these pages are so consistent that they can be very briefly summarized.

Firstly, the introduction of an empty gap between the stopping point of the motor and the starting position of the patient does not destroy the "causal impression." The chances of producing such an impression, however, decrease as the size of the gap increases. Distance is, thus, one condition, among others, which determines the production of the phenomenon.

Secondly, phenomenal causation at a distance has proved to depend on the same patterns of stimulation as causation with contact and to be affected in the same way by the same changes of the stimulus conditions.

The distance, though, seems to have a slight disintegrating effect on the configuration, as is indicated by the fact that the objective conditions (time interval between the two movements, absolute velocity and velocity ratio) must be somewhat more favourable in the case of distance than in the case of contact. But, when these conditions are a great deal more favourable for the case of distance, the causal impression is also more satisfying in this case.

And thirdly, when the impression is reported, it seems to be similar to that produced by spatial contiguity. This fact is objectively confirmed by the finding that the size and variability of the RA—an essential feature of the impression—have practically the same values irrespective of the presence or length of the gap.

It seems, thus, that on the whole, although static distance is a condition determining the production of the phenomenon, it is not more a constituent of the perceptive causation structure than, let us say, the surrounding space.

Theoretical Implications

1. It scarcely needs to be pointed out that the facts we have described are obviously contradictory to our everyday experience and to our acquired knowledge concerning mechanical causation. Everybody knows perfectly well that it is not sufficient to stretch one's arm in the direction of a ball to put it in motion, or to hammer in the air in order to drive a nail.

It is thus very unlikely that the causal impression could be due to an influence of past experience. Besides, how could past experience explain why the causal impression is bound in such a systematic way to its objective conditions, even in such an "absurd" situation as the one we have studied here? Past experience can, and very likely does, influence the impression. It can hardly account for its specific and fundamental aspect, the pushing itself.

2. It is also clear from our results that any theory maintaining that there is, in the case of phenomenal causation, a "passage" of the movement from the motor to the patient would be hardly defensible. With such a "passage" the movement would take a certain time to reach the patient, and this time would increase with the separating distance; which is by no means what really happens, as we have seen.

3. Our findings show, furthermore, that the former opinion of Michotte concerning the intimate connexion between the causal impression and the impression of "going away from" (effet écartement), although it seemed to hold in the case of contiguity which he almost exclusively studied, should now be revised. Thus, we have found that the "effet écartement" was impossible to obtain in the case of distance (especially with greater distances), whereas the impression of pushing was reported as evident.

The similarities which Michotte has shown to exist between the RA's for causation and for "écartement" do not necessarily mean that there is an internal connexion between these phenomena. They may be due to some common law governing all sorts of changes which the aspects of movements may undergo under certain conditions. This view seems to be strongly supported by other researches carried out at Louvain by the present author (Yela, 1952).

4. As to the theory of "ampliation," it is not our intention to discuss it at length here, since Professor Michotte himself is planning to publish shortly an article on his theory in the light of many new observations made by himself and his co-workers.

It will now suffice to point out that our results do not raise any serious difficulty and to show in a few words how they would be related to such a conception.

Supposing the reader to be acquainted with the general outline of the theory, we will summarize it as follows. Due to a special stimulation pattern consisting of two successive movements taking place in immediate succession, in the same direction and at convenient speeds, but carried by two different objects, a specific perceptual configuration is produced. The main features of this configuration would be that the two motion processes (therefore *even* the one performed by object B, the patient, but this only for a short time) would phenomenally *belong* to the motor object A. During that short period, the movement actually performed by B would, thus, not appear as its own and, hence, B would seem to be moved by the previous movement of A, i.e. by the shock it has received from it.

As may easily be understood, the essential point of the theory is the *transitory belonging* to A of the movement performed by B.

Now, granted that a movement may seem to belong to an object different from that which actually moves, the new question which arises from our experimental data is whether this could occur even in the case of a more or less distant object. In other words, it poses the problem of *belonging at a distance*. But this problem does not seem really to make any difficulty. Many examples of such belonging without spatial contiguity may be found in daily experience, even in the case of static patterns as, among others, those of mathematical symbols, e.g. those of the square root, exponents, the case of a point marking the centre of a circumference, and the like.

Furthermore, in experiments arranged in such a way that a part of an object, of a simple geometrical form, for example, moves away from the rest up to a relatively considerable distance, it goes on appearing as "a part of the whole" a fairly long time, before it looks as a "closed" form of its own.

Finally, that the distance is practically irrelevant to the causal impression, can be easily understood. Thus, the perceptual configuration of causation consists of two *processes*, which are intimately integrated so as to form phenomenally one new global process, a "unitary operation." All we know about it so far indicates that the producing of this new process is essentially bound to temporal and kinematic properties, as immediate succession, velocity and direction of the movements, and the like. These properties are very profoundly different from purely static ones, like a simple distance, and, thus, it is perfectly understandable that their integration could eventually be largely independent from these.

Movement properties like velocity and direction can, it is true, be represented by vectors, but it must be kept in mind that this is purely symbolic. These properties when "experienced" in the perception of motion have an evolutionary character that gives them a very close kinship to purely temporal configurations. It is precisely this last point which is emphasized by our results, since they show that in causation the perceptual pattern proves to be practically as independent on static distance as temporal configurations like succession or simultaneity.

Acknowledgment

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RELATIONSHIP OF MOTIVATION AND METHOD OF TESTING TO DISTRIBUTION OF PRACTICE PHENOMENA IN ROTARY PURSUIT

BY

ROBERT B. AMMONS

(*Department of Psychology, University of Louisville*)

Women college students (N = 120) practised 60 r.p.m. rotary pursuit for 12 minutes, rested for approximately 25 minutes and then practised for 12 minutes more. During the initial period, thirty-six 20-second trials were given, 20 subjects each practising with intertrial rests of 0 seconds, 20 seconds, 50 seconds, 2 minutes and 5 minutes. In the 12-minute post-rest or testing period, 10 subjects in each of these groups practised continuously, and 10 were given 20-second trials separated by 5-minute rests.

Greater distribution was found to lead to better performance in the initial period, except that the 5-minute inter-trial-rest group did not perform as well as the 2-minute group.

The warm-up decrement was dependent on the degree of distribution of practice in the initial period, being greater with greater massing of trials. As measured by the backward-extrapolation method, less was shown when testing practice was continuous than when it was distributed.

The temporary work decrement was far greater in magnitude than the permanent work decrement. If permanent work decrement was present at all, it was small in magnitude; approximately one-half was due to the conditions under which it was tested for, rather than to the degree of distribution in the initial stages of practice.

I

INTRODUCTION

Ammons (1947*a*) and Melton (1941) noted a wavelike curve following rest in relatively continuous rotary pursuit. This curve consisted of a sharply rising portion, then a relatively falling segment, followed by a section in which a rise comparable to that in the pre-rest curve was resumed. Ammons proposed that the initial rise was due to a need to warm-up (D_{wu}), the falling segment was due to an accumulation of temporary work decrement (D_{wt}), and the curve resumed its rise after this temporary decremental factor had reached its maximum. These variables were given quantitative operational definitions and a miniature system was constructed in a guess at their relationships.

As temporary work decrement disappears over a rest, and warm-up decrement affects only the first few minutes of resumed practice (Ammons, 1947*b*), permanent work decrement (D_{wp}), is the only factor leading to important persisting differences in performance within the system. For this reason, guesses were made as to the conditions under which it would develop, and an empirical study was carried out (Ammons, 1950) to determine its magnitude as a function of degree of pre-rest distribution of practice with post-rest distribution held constant for all groups. It was predicted that the greater the pre-rest massing, the more D_{wp} developed, and hence the lower the final level of performance. A complex relationship was found between final level of performance and amount of initial distribution, with best performance with 50-second or 2-minute rests between trials, and poorer performance with 0-second, 20-second, 5-minute, and 12-minute rests.

Results from this last study were felt to be due possibly to neglected variables, the operation of which would bear further investigation. (*a*) *Motivation.* It has been proposed (Hull, 1943; Kimble, 1949; Kimble and Bilodeau, 1949) that differences in level of performance with differential distribution of practice are due to differences in motivation. On the basis of casual observation, a high level of motivation was felt to have led to disproportionately good performance in one group having highly distributed trials (24-hour rests between 20-second trials). If motivation is a factor in the superiority of distributed practice groups, then self-ratings of motivation should vary systematically with amount of distribution. (*b*) *Method of testing*. It will be recalled that best final performance was found with intermediate levels of distribution. This suggests that there may have been two variables involved, one producing poorer performance with greater massing, and one producing poorer performance with greater distribution. Poorer performance with greater massing could be attributed to permanent work decrement, as predicted. Poorer performance with greater distribution could have been due to a generalization effect. It has been demonstrated with conditioned responses (Hovland, 1937) that the less similar a test stimulus is to the original conditioned stimulus, the weaker the conditioned response elicited. Testing was done in Ammons' experiment with continuous practice. If generalization was operating, we would expect that the greater the initial distribution, the less similar the test condition was to the training condition, and consequently the poorer the test performance. A simple check on this relationship could be made by repeating the experiment and testing part of each of the initial distribution groups under distributed practice and part under massed. If generalization were an important factor, we would expect to find that with massed testing, the initially massed groups would do relatively better, while with a distributed testing condition, the initially distributed groups would do relatively better in the testing condition. Thus the constant distribution condition in which the effects of initial distribution differences were tested might prove to be important.

II

Problem

This experiment is set up to test the following hypotheses: (*a*) after initially different distributions of practice, final relative levels of performance are not affected by the distribution of practice during the testing period, providing it is the same for all groups; (*b*) initially different distributions of practice have no effect on the final level of performance when distribution of practice during the testing period is the same for all groups; and (*c*) motivation is not systematically related to distribution-of-practice phenomena.

III

Experimental Arrangements

Subjects

One hundred and twenty college students served. They were volunteers, and naïve to the apparatus. The records of several additional subjects were discarded during the experiment because of apparatus failures. No subjects were lost because of refusal to continue. There were 12 in each of the 10 experimental groups, assigned as available, not randomly. There was no known systematic bias in this. Table I includes the results of analysis of variance of scores on the first 20-second pre-rest trial. The hypothesis that the groups were chosen randomly from the same population cannot be rejected. No matching was attempted, in view of the relatively small number of subjects.

Apparatus

The same 4-unit pursuit rotor as described in a previous paper (Ammons, 1950) was used. Black vinylite turntables were 10¾ in. in diameter, with ¾-in. brass targets set flush with their surfaces, centres 3¼ in. from the centres of the turntables. These revolved clockwise at 60 r.p.m., and were checked for speed at least every 24 minutes of operation. Silver-tipped styluses were 5¾ in. long from hinging point to tip bend, and rested with only part of the weight of the outer ⅛-in. rod (0·6 oz.) on the turntable surface. Circuit completions (6 V. D.C.) were recorded as to duration by 0·001-minute Standard Electric timers. Two banks of 4 timers each were wired in and used alternately to permit recording of scores during continuous practice. Trial intervals were controlled manually, because of the complexity of the conditions. The stylus tips and targets were cleaned with carbon tetrachloride and crocus cloth before each 12-minute session. Illumination was always greater than 15 foot candles.

Procedure

The subjects were told before the experiment that the test was one of co-ordination, had been used for pilot selection during the war, and that they could see how the clocks and control panel worked after they had finished. They knew the total duration of the session. They were given instructions as follows: to keep the tip of the stylus on the target as much of the time as possible, to follow the target with an easy swinging movement, and not to expect to do very well at first but to try to improve.

A light palm-vertical grip on the stylus was demonstrated and subjects' grips tried and corrected where necessary, with the further comment that they were not to try to press down on the stylus tip in any way. Questions were answered, and practice begun. Deviations from instructions during practice were corrected by further verbal instructions.

Almost all the subjects practised in groups of four, pairs facing away from each other in a "V" with the experimenter at the tip. Groups were sandwiched where intertrial rests permitted, so that eight or twelve subjects were run in relays, with non-practisers leaving the room.

Trials consisted of a verbal warning, followed after 3 seconds by the rotors' starting to revolve. After 2 more seconds, the clocks were turned on for 20 seconds. In continuous practice, clocks were cut out of the circuit for 2 seconds before each 20-second trial, although the rotors turned continuously. Trials were of the same length (20 seconds) for all groups, with only the intertrial rests varied, since varying both trial length and rest length would have a complex effect, if Kimble and Bilodeau's conclusions are applicable to rotary pursuit.

All practised 24 minutes, two 12-minute sessions being separated by a 13- to 30-minute rest. Rests averaged 22 to 25 minutes for the various groups and were varied because this made it possible to run several groups of four at a time. Rests of over 10 minutes have been shown by Ammons (1947*b*) to have an essentially constant effect on warm-up decrement and temporary work decrement. Practice periods of 12 minutes were used because in continuous practice temporary work decrement builds to somewhat near a maximum by this time and stable levels are obtained (Ammons, 1947*b*).

The ten conditions represented every combination of five pre-rest distributions of practice and two post-rest testing distributions. Before rest, 0-second, 20-second, 50-second, 2-minute or 5-minute intertrial rests were given, while, after rest, half of each of the pre-rest groups were given 0 second (continuous practice), and half 5 minutes between trials. Thus the effects of each pre-rest level of distribution were tested in a massed and in a distributed post-rest condition.

Conditions are identified in terms of the intertrial rest in pre- and post-rest practice. 20–5 refers to the condition where 20-second rests were given in the initial 12-minute period and 5-minute rests in the testing period. Following this scheme, the ten conditions can be symbolized as follows:

0–0
20–0
50–0
2–0
5–0

0–5
20–5
50–5
2–5
5–5

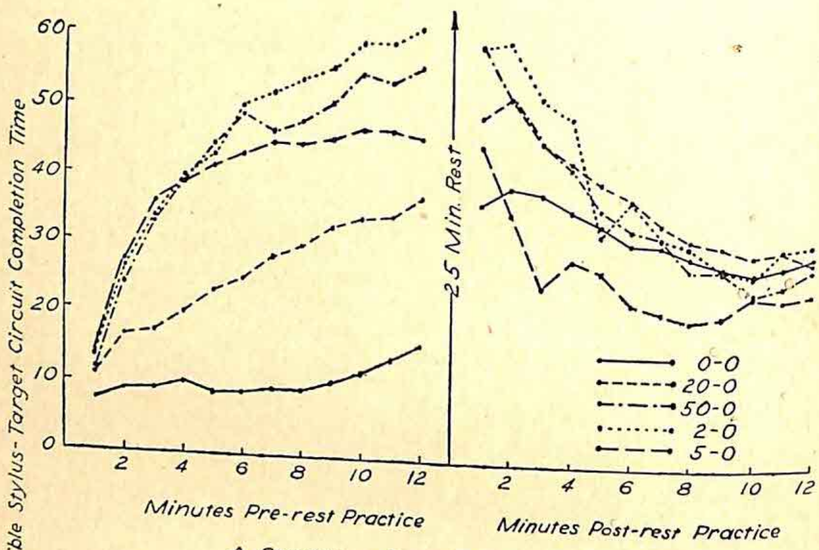
During practice, motivating remarks were made approximately once a minute for all groups, as a tendency had been noted to encourage distributed groups more than massed. Subjects read, talked or drank soft drinks during the longer rests.

Before and after practice subjects rated their motivation. They were told that the level of the rating meant nothing to the experimenters, and that it was very important that it be accurate and honest. The pre-practice sheet read: "I want to score: (1) better than I ever did before on similar tasks; (2) the very best I can; (3) about as well as I can; (4) above average, but not knocking myself out; (5) about average; (6) just enough to be an adequate subject." The post-practice sheet read: "As compared to other similar tasks I have engaged in, I tried: (1) very hard; (2) fairly hard; (3) somewhat more than usual; (4) little more than usual; (5) average; (6) somewhat less than usual; (7) little." The subject merely checked the alternative which she felt best represented her feelings at the time.

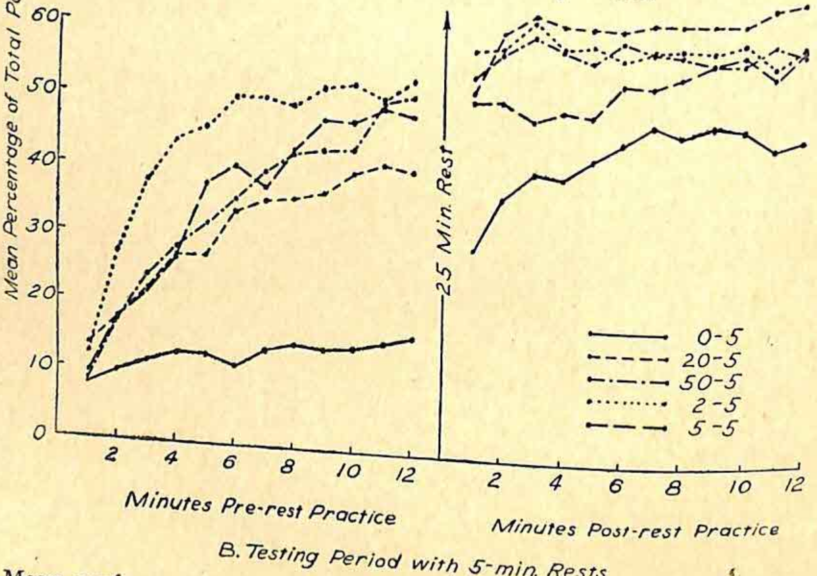
IV

Results

The first step in analysis was to calculate mean clock scores for the 10 groups by 1-minute periods. These values are plotted in Figure 1. It can be seen that during



A. Continuous Practice Testing Period



B. Testing Period with 5-min. Rests

Fig. 1.—Mean performance of the ten experimental groups by 1-minute periods, continuous and 5-minute rest testing groups plotted separately.

the initial practice period performance is better, the greater the distribution of practice. The only exception is the 5-minute rest group which is poorer than the 2-minute rest group, as was found in a previous study (Ammons, 1950). This probably indicates that for this task there is an optimum level of distribution of practice with rests shorter than 5 minutes.

Examination of the curves for the testing or post-rest practice periods shows a general tendency for the groups tested in the 5-minute distribution condition to show a rise to a steady level. Those tested in the continuous practice condition show a marked fall to a steady level. These overall post-rest differences are due mainly to the development of temporary work decrement during massed practice (Ammons, 1950).

Analyses of variance were made, to evaluate performance variables at different stages in practice. The variance estimates obtained are shown in Table I. The first analysis was of performance scores during the first 20-second practice period, where distribution had not yet been introduced to influence the scores. As has already been mentioned, there is no basis for rejecting the hypothesis that the groups represented random samples from the same original population. It should be noted, however, that the score distributions at this point were considerably skewed.

TABLE I

VARIANCES ESTIMATED FROM ANALYSIS OF VARIANCE AT VARIOUS POINTS IN PRACTICE

		Variances: Estimated from		
Source of variance estimate	*d.f.*	*First 20 seconds of pre-rest practice*	*First 60 seconds of post-rest practice**	*Last 200 seconds of post-rest practice†*
1. Between groups	9	239·9	120·6	356·8
1*a*. Length of pre-rest intertrial rest	4		256·5	33·8
1*b*. Length of post-rest intertrial rest	1		0·7	2959·6
1*c*. Interaction of 1*a* and 1*b* . .	4		14·7	29·2
2. Within groups	110	495·7	17·2	14·0

* Computations done with raw scores in 0·001 minute for this period coded by dividing by three.

† Raw scores coded by dividing by ten.

The effect of initial distribution of practice on warm-up decrement (D_{wu}) after a rest

The second analysis of variance was of scores for the first 60 seconds of post-rest practice. This period was chosen as giving a rough estimate of amount of warm-up decrement. Table I shows that there was no statistically significant interaction between the effects of pre-rest distribution level and post-rest testing distribution level, and no significant effect from post-rest distribution level. The effect of pre-rest level of distribution is highly significant, with an F of 14·9 and 4 and 110 degrees of freedom.

Inspection of the curves in Figure 1 in general bears out these findings as indicating changes in D_{wu}. Warm-up decrement has been formally defined as the difference between the obtained level on post-rest trial 1 and the level estimated for this point by backward extrapolation of a line fitted to the relatively less sharply rising segment of the post-rest curve (Ammons, 1947*a*). In the case of the present curves, this line would be fitted to approximately the points from minute 2 to minute 10. By inspection, it can be seen that for the curves in the continuous practice testing condition there is appreciable warm-up decrement only for pre-rest groups having 0-second and 20-second intertrial rests. In the 5-minute intertrial rest testing condition, those pre-rest groups with 0-second, 20-second, and 50-second intertrial rests during initial practice show definite warm-up decrements.

It is also apparent from visual inspection of Figure 1 that the warm-up decrements measured by the backward-extrapolation method are much larger for the 5-minute rest than for the continuous-practice testing condition. This may be due to the fact that the trial scores for the first minute of continuous testing practice include in effect one distributed trial, the first 20-second trial after rest. They are thus relatively higher than those at later points in continuous practice. Actual determinations of the sizes of the decrements have not been made because of the small number of cases and because the calculation of these values is not necessary in the testing of the present hypotheses.

As estimated by the backward-extrapolation method, testing condition markedly affects warm-up decrement. Both methods of estimating warm-up decrement indicate that with testing condition held constant, warm-up decrement is due almost completely to amount of massing during previous practice, with more decrement following greater massing.

Effect of distribution of practice during initial and testing periods on final level of performance

A third analysis of variance was made of the scores from the last 200 seconds of post-rest practice. It was felt that these values would reflect any permanent work decrement due to differential massing of practice during the initial 12-minute period. The formal definition of permanent work decrement is in terms of scores corrected for temporary work decrement and warm-up decrement as compared with optimum performance, in this case that of the 2-minute pre-rest intertrial rest group practising under the 5-minute distribution testing condition.

The final levels reached by all the groups in this distributed testing condition meet the conditions of the definition, since warm-up decrement has been overcome, and no temporary work decrement allowed to develop. Except for temporary work decrement, the groups in the continuous practice testing condition also meet the conditions of the definition. Since warm-up decrement is not present in the final level for this group, and temporary work decrement is approximately constant for all groups (Ammons, 1950), group differences in level will be due to permanent work decrement based on initial differences in distribution of practice or testing method, or an interaction of the two factors.

Analysis of variance of the scores during the last 200 seconds of testing practice allows an evaluation of the relative importance of the factors influencing final level of performance. The various variance estimates appear in the last column of Table I. The F-ratio of 25·5 based on the between-groups and within-groups variances is significant at well beyond the 1 per cent. level of confidence, so the between-groups variance can be meaningfully broken down. There is an interaction ($F = 2{\cdot}09$) significant at approximately the 10 per cent. level between degree of initial distribution and degree of distribution in the post-rest testing period. When the interaction variance is used to test the main effects the degree of pre-rest distribution does not have a significant effect, while testing distribution very nearly accounts for the total between-groups effect. The interaction variance and variance due to differential initial distribution have nearly the same magnitude.

From these results and inspection of Figure 1, it can be generalized that if there is any permanent work decrement, it is not proportional to the amount of temporary work decrement present in the initial practice period. What little permanent decrement may be present appears to be about half due to an interaction of initial and testing conditions of practice, affecting only those groups (0–5, 5–0) changing from the extreme of massing to the extreme of distribution, and about half due to some sort of pemanent distribution effect. By far the major effect during both practice and testing periods is that of temporary work decrement.

Relationship of self-rated motivation to distribution of practice phenomena

It will be recalled that subjects rated their own motivation levels before and after practice. Table II shows the mean ratings by groups before and after practice. The values only provide indices, since the scales did not have predetermined equal-appearing intervals, and differed in the items used. General level of motivation was excellent, although the initial continuous practice groups were apparently somewhat poorer in this than the other groups.

TABLE II

MEAN MOTIVATION SELF-RATINGS* OF THE GROUPS BEFORE AND AFTER PRACTICE

Group	*Mean motivation self-rating*	
	Pre-practice	*Post-practice*
0–0	3·25	2·67
20–0	2·50	1·50
50–0	2·25	3·08
2–0	2·80	2·67
5–0	2·08	2·08
Mean	2·58	2·40
0–5	3·75	3·25
20–5	2·00	2·33
50–5	2·16	2·25
2–5	2·50	1·50
5–5	2·00	1·50
Mean	2·48	2·17

* The smaller the figure, the higher the motivation.

The results of analyzing the variance of pre- and post-practice ratings are presented in Table III. The only significant effect on pre-practice motivation was related to

TABLE III

VARIANCES ESTIMATED FROM ANALYSIS OF VARIANCE OF SELF-RATINGS OF MOTIVATION

Source of variance estimate	*d.f.*	*Variance estimated from pre-practice ratings*	*Variance estimated from post-practice ratings*
1. Length of pre-rest intertrial rest	4	8·24	6·10
2. Length of post-rest intertrial rest	1	0·40	2·41
3. Interaction of 1 and 2	4	0·87	4·65
4. Within groups	110	0·72	1·95

pre-rest conditions. After practice the within-groups variance was significantly larger than before practice (F=2·71, with 110 and 110 degrees of freedom). This may have been due to the change in rating scale, or to an actual increase in variability of ratings. There was an interaction between initial and testing distributions significant at not quite the 5 per cent. level (F = 2·38 for 4 and 110 degrees of freedom). Using the interaction variance as the error variance, neither main effect is significant at even the 10 per cent. level. With the within-groups variance as error term, the pre-rest intertrial rest had a significant effect (F = 3·12 for 4 and 110 d.f.).

Why was there a significant difference in motivation between groups before practice was even started? Subjects did not know the condition under which they would practice, so this knowledge could not have influenced them. Total length of practice period could not have produced the effect. It was known to subjects, but those practicing in the distributed testing condition practised two or more times as long as those in the continuous testing condition, yet were just as motivated. Variables related to time, such as changes in time available with approaching end of semester or apparatus changes were ruled out by an inspection of the dates when the various conditions were run. High and low mean motivation levels were found for some conditions run early in the experiment and also for some run late. It can be speculated that the cause of the pre-practice motivation differences lay in an attitude on the part of the experimenter, of which he was not aware.

In view of the data and argument presented above, certain generalizations as to the relationship between motivation and performance seem reasonable. Length of practice period had little effect on motivational levels. Although perhaps easily influenced by the experimenter's attitudes and moods, motivational levels remained good for long complicated conditions, and apparently were little related to level of performance in any case. No consistent relationship was found between degree of distribution of practice and motivation, so that theories attempting to account for distribution of practice phenomena in terms of changes in motivation are given no support.

V

Discussion

Perhaps the single most important finding here reported is that what small "permanent work decrement" was present was no more than half due to previous massing of practice and was not proportional to the degree of this massing. Numerous theories have attempted to account for distribution phenomena in terms of two decremental factors, one (labile) which dissipates over rest, producing reminiscence, and one (stable) which cumulates and handicaps the group having massed practice, even after rests. Such theories as those of Hull (1943), Kimble (1949), and Ammons (1947*a*) are of this kind. These theories must be revised if the present findings are confirmed by further experimentation.

Kimble and Hull have considered the temporary decremental factor to be at least partly motivational in nature, a drive whose goal is the cessation of practice. The present findings of no consistent relationship between distribution of practice phenomena and motivation call these formulations into question. It is possible that if subjects were given a great deal more practice with more difficult tasks, permanent decrement due to motivation or some other factor would appear. This possibility remains to be explored in further experimentation, as well as by re-examination of other distribution of practice studies, in each case taking the necessity for a common testing task into consideration.

Objections can be raised to conclusions from this study on several grounds. Because of the potential theoretical importance of the study, these objections will be discussed in some detail.

(*a*) *Initial statistically significant group differences in motivation make further motivation analyses valueless.* Some reasons for the differences have already been considered. It is even conceivable that they are due to chance. In any case, they are not systematic with respect to degree of distribution, as can be seen in Figure 1 and Table II. That the initial and final rating scales are sufficiently reliable to show group differences is apparent from the fact that initial and final motivation self-ratings correlated +0·60 (median, rank order) within the groups of four subjects. Initial ratings correlated +0·50 (median, rank order) with rotor scores on the first 20-second trial, while final ratings correlated +0·40 (median, rank order) with rotor scores for the last 200 seconds of practice. Thus, self-ratings of motivation are related to performance level, are sufficiently reliable to reflect group differences, and their differences are not systematically related to the distribution conditions on the first trial. If distribution effects were due to motivational differences, these motivational differences should be detectable in the final self-ratings with the same efficiency as if there had been no statistically significant difference between the groups in initial self-ratings.

(*b*) *Experimental groups of twelve subjects cannot be considered as random or initially comparable, and consequently the rotor data cannot be analyzed meaningfully.* It is a fact that subjects were assigned to conditions in groups of four and as available, and so, formally, were not assigned randomly with respect to either time of testing or condition. A subjective review of the conditions revealed no detectable bias in assignment of subjects to conditions. A statistical check on the randomness was also made. The variance within the experimental groups of twelve subjects was separated into two components, variance within groups of four and variance between groups of four. A comparison of these two variances shows the degree to which groups of four are more homogeneous because of being handled as a unit than they should be if strictly random. The variance estimate for within groups of four was 33·9, while that for between groups of four was 32·8. From this it can be seen that the members of groups of four can reasonably be treated as if they had been independent.

A different check can be made by analyzing the total variance of scores on the first 20-second trial, when groups all received the same treatment as already reported. Table I shows that the experimental groups did not differ by more than could reasonably be attributed to chance. That these initial scores are adequate indices of over-all performance is indicated by their correlation (median, rank order) within groups of twelve of +0·70 with scores on the second 20-second trial, and +0·40 with scores for the final 200 seconds of practice. On these bases, it can be concluded that the groups can be treated as if random and as initially comparable.

(*c*) *The experimental groups are too small to permit conclusions.* In planning this experiment, the most efficient design was sought. The present design allows for the independent estimation of the effects of distribution on temporary and permanent work decrement and the effects of method of testing. If the number of subjects had been increased from twelve to fifty in each experimental group, and the within groups variance used as an error term, raising its degrees of freedom from 110 to 490, the following changes would have taken place in the sizes of F ratios necessary for significance at the 1 per cent. level of scores from the last 200 seconds of practice. The F ratio for temporary effects (1*b* in Table I) would decline from 3·93 to 3·86, while those for permanent effects (1*a* in Table I) and for testing method or the interaction between temporary and permanent effects (1*c* in Table I) would decline from 2·46 to 2·39. If the interaction were used as the error term, there would be no direct gain by increasing the number of subjects. It would seem that the design is quite sensitive where this is needed, and that increasing the number of subjects would accomplish little or nothing.

From a practical point of view, for the purposes of the present experiment, the effects of initial differences in motivation are unimportant, the experimental groups can be treated as essentially random and comparable, and the experimental groups are large enough for all the important comparisons to be made with satisfactory precision.

VI

Conclusions

On the basis of the findings in the present study, the following tentative conclusions seem warranted concerning the rotary pursuit skill in the early stages of acquisition:

(*a*) distribution of practice theories postulating a large permanent work decrement need revision;

(*b*) distribution of practice theories based on changes in motivation paralleling changes in performance level due to degree of distribution need revision;

(*c*) re-examination of previous distribution studies and further experimentation are called for to determine if there is *any* relatively permanent decremental effect associated with massing of practice, and if so, the conditions under which it develops.

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VARIATIONS IN BLINK RATE DURING NON-VISUAL TASKS

BY

R. L. GREGORY

(From the Medical Research Council Applied Psychology Research Unit, Cambridge)

A total of fifty-five subjects were tested in three experimental situations. Blinking was recorded electrically, and related to various features of the three non-visual tasks. A new type of suction electrode was developed for this purpose.

It was found that blink rate fell below the normal resting rate during the learning of a stylus maze, and during a special audio-tracking task. During pauses between each trial, blink rate was above the resting rate.

There was a significant tendency for those subjects who showed the greatest difference in blink rate during the trials and the pauses between each trial to make the fewest errors per run of the stylus maze.

In the audio-tracking task the subjects followed a changing note with a second oscillator. Blink rate was found to be inversely related to the rate of change of the course tone.

PURPOSE

It has been found by a number of experimenters, for example, Ponder and Kennedy (1927), Carpenter (1948) and Drew (1950), that blink rate tends to vary with changing experimental conditions. This has suggested the possibility that blink rate might be used as an index of some general psychological unobservable, such as Attention or Effort.

Ponder and Kennedy were the first to deny that blinking is reflex; they showed that no afferent stimuli are necessary for normal blinking, and argue that it is probably innervated from the region of the basal ganglia. This suggests that subtle modifications of blink rate with changing task conditions is at least plausible on physiological grounds.

Most previous work has investigated blinking in relation to visual tasks. It has been regarded as important for studies on reading (Luckeish and Moss, 1942), and as a possible cause of accidents through loss of information (Lawson, 1948). Drew showed that blink rate decreases as the necessity for detailed control of movement increases; he found this both for a laboratory tracking task and for driving a car in various traffic conditions. A reduction in blink rate associated with increase in difficulty of a tracking task was also found by Poulton and Gregory (1952).

This present series of experiments was undertaken in an attempt to discover whether blink rate is associated with changes in the conditions of non-visual tasks.

II

CHANGES IN BLINK RATE WHILE LEARNING A STYLUS MAZE

The task was the learning of a small stylus maze which was hidden from the subject by a screen. The maze had a groove cut in a wooden board 10 in. long by 5 in. wide. It had nine culs-de-sac, each 1 in. in length and regularly spaced along the maze; five were on the right, and four on the left. All the turns were right angles; there were no curves in the maze. It was placed horizontally on a table; the subjects put their right arm round a small screen which hid the maze but did not otherwise restrict their field of view.

Blinks were recorded with an electromyograph consisting of a direct-coupled E.E.G.-type amplifier feeding an ink-writing oscillograph. Electrodes were placed above and to the side of the left eye.

Errors were recorded on the same record as the blinks. The experimenter depressed a key when the subject's stylus touched the end of a cul-de-sac, a record thus being made of errors which could be directly related to the blink record. The beginning and end of each trial were also recorded.

In this and the other experiments to be described, the experimenter resorted to a trick in order to reduce the risk of the subjects realizing that blinks were being recorded. The subjects were told that their brain activity was being electrically recorded. The subject did not at any time see the maze; he was told that he must not see it because, if he did, the task would be absurdly easy. No reference to blinking or to the eyes was made at any time.

Twenty naval subjects served as subjects; their ages ranged from 19 to 25, except for one who was 35 years of age. They had various ground-crew jobs on aircraft.

Procedure

When the electrodes were placed in position, and the recorder was operating satisfactorily, the subjects were told to relax for a 2-minute period. A similar period followed the learning of the maze to a criterion of three correct runs. These two periods provided some measure of the normal rest rate for the subject. There were ½-minute pauses between each trial, this had the advantage that these "pause rates" could be compared with the beginning and end "rest rates." It also had the advantage over the vigilance type of experiment that changes in blink rate during the course of the experiment, perhaps related to such variables as boredom or fatigue, could be ignored or separately studied.

The records were scored by dividing the learning trials and the pauses into ¼-minute periods which were averaged separately. This was done partly because the first half of the maze was learned very quickly; it was only the second half which seemed to present difficulty after the first two or three trials.

Results

The pause rate was markedly higher than the rest rate; the trial rate was below the rest rate. Blink rate decreased during each trial, and it also decreased during each pause between the trials.

The difference between the trial rate and the rest rate (the smallest difference) is significant at the 0·05 level. When the second half of the trial periods is considered, the difference is significant at the 0·001 level. The difference between the pause and the trial rates is highly significant at the 0·001 level. The figures are given in Table I.

TABLE I

STYLUS MAZE EXPERIMENT

		PAUSES			TRIALS			
	Rest 1	*1st ¼ min.*	*2nd ¼ min.*	*1st and 2nd ¼ min.*	*1st ¼ min.*	*2nd ¼ min.*	*1st and 2nd ¼ min.*	*Rest 2*
Means: ..	21·52	33·04	27·36	30·2	22·32	15·00	18·66	20·24
S.D. ..	2·49	3·68	2·99	3·08	4·12	4·97	6·05	5·39

An attempt was made to find a relation between blink rate and errors. The method adopted was to rank the twenty subjects in order of the difference between trial (T) and pause (P) blink rate, and also in order of the number of errors made per trial. The correlation between these ranks was then assessed using Kendall's τ test. Clearly, a correlation coefficient of nearly zero would indicate that there was no relation between errors and the difference between pause and trial blink rate for each subject, while a correlation of 1 would indicate that in each case the subject with fewest errors per trial had the greatest reduction in blink rate.

It was found that, when P–T was ranked and correlated with mean errors per trial for each subject, τ was 0·37.

When only the first $\frac{1}{4}$-minute period of the trials was taken for T, τ was 0·48. The difference between the rest rate and the mean trial rate was also ranked for each subject. This gave a τ of 0·24.

III

CHANGES IN BLINK RATE WHILE ADDING NUMBERS

This experiment was undertaken partly to find out whether the motor activity present in learning the stylus maze was an important factor in modifying the blink rate; although this seemed unlikely, it was a possibility. A further reason for varying the task was that some subjects learned the maze by building up a visual picture; it seemed just possible that to these subjects the task might be in a sense "psychologically visual," even though information was not lost through blinking.

Procedure

Single digit numbers chosen randomly were recorded on a magnetic tape-recorder in four series of twenty digits each. The subjects were asked to add each series mentally, and to give the answer verbally at the end of each series.

A further twenty naval ratings took part in this experiment. The same blink recorder was used, but electrodes were developed which, instead of being fixed with sticking plaster as in the stylus maze experiment, were held in place with suction.

The Suction Electrodes

A special electrode was designed for this purpose; it consisted of a small hollow copper cylinder across the open top of which was stretched a thin rubber diaphragm. When the electrode was attached, this diaphragm was pushed into the hollow electrode with the finger so that the air was expelled through a small hole in the centre of the electrode face. The diaphragm was released when the electrode was in place, and the resulting vacuum held it firmly to the skin. A sticky electrode jelly was used which besides ensuring adequate electrical contact also sealed the vacuum inside the electrode. The advantage of this method over the use of sticking plaster is that it is less unpleasant for the subject; also the electrode may be readily transferred from one place to another without coating the skin with gum, which is annoying since it increases the electrode resistance. Most important, it improves the quality of the records because the electrode is held firmly at its centre, whereas if it is held with plaster it tends to work loose.

Results

The blink rates (number of blinks per minute) were calculated for four experimental conditions. These were: (1) a 2-minute Rest period at the commencement of the experiment; (2) the same period at the end of the experiment; (3) the average of the Trial periods for each subject; (4) the average of the Pause periods for each subject. The Means for the twenty subjects in these four conditions are: (1) $R_1 = 17{\cdot}84$; (2) $R_2 = 22{\cdot}88$; (3) $T = 20{\cdot}62$; (4) $P = 25{\cdot}06$.

An analysis of variance showed that the difference between the means of (3) and (4) were highly significant at the 0·001 level. The results of the analysis are shown in Table II.

TABLE II

NUMBER ADDING EXPERIMENT

Item	*d.f.*	*S.S.*	*M.S.*	*F. ratio*	*p.*
Subjects	19	1406·27	74·01	10·11	<0·001
Conditions	3	143·75	47·92	6·55	<0·001
Error	57	417·49	7·32		
Total	79	1967·51			
S.D. 2·7					

IV

Changes in Blink Rate during Audio-Tracking

This experiment was undertaken in an attempt to discover whether blinking is related to the rate-of-change of a non-visual tracking course.

Apparatus

Two audio-frequency oscillators were built, employing the Wein–Bridge frequency selective network principle. This allowed the frequency of each oscillator to be varied by mechanical rotation of a single potentiometer. Each oscillator fed a high quality 5-in. speaker; these were mounted on boards one at each side of the subject's head. The two notes could thus be recognized from their direction.

The oscillator feeding the right-hand speaker was continuously varied in frequency, its frequency-control potentiometer being connected by a string drive to a crank driven by a motor through an integrating disc. The integrating disc was used as a continuously variable gear to set the period of the simple harmonic course produced by the crank driving the potentiometer.

Both the course and the subject's response to it were recorded by ball-pointed pens in slides mounted across the record paper and in line with the blink recording pen. The ball pens were mounted at an angle of about 40° from the vertical, so that their points almost touched when they passed each other. One of the pens was mechanically connected to the crank driving the frequency-control potentiometer of the course oscillator, the other pen was connected to the corresponding potentiometer of the response oscillator, which was controlled by the subject with a 2-in. control knob connected through a reduction gear. This gear was introduced to make it impossible for the subject to develop a stereotyped hand movement; the knob had to be turned through rather more than 360° to follow the course oscillator.

The subject's task was to follow the simple-harmonic changes in frequency of the right-hand speaker by producing corresponding changes in the left-hand speaker by rotating the control knob. All the moving parts of the apparatus were screened from the subject in order to avoid visual cues.

Results

The blink record was related to the tracking course by dividing each sine-wave drawn by the course pen into eight equal sections. The number of blinks falling in each section was determined for each sine-wave in turn, and these were totalled for each record and expressed as a percentage of the total number of blinks for each subject.

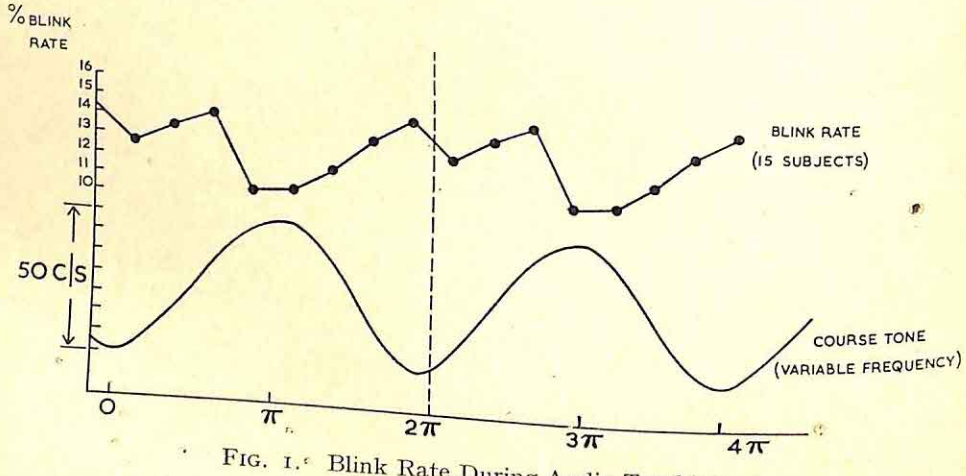

FIG. 1. Blink Rate During Audio Tracking.

Figure 1 shows the average of the results obtained in this way for fifteen subjects. This was obtained by expressing their contributions as a percentage of their tracking time to the total.

Each subject is given an equal weight, although both the average blink rate and the tracking time have between-subject variation.

From Figure 1 it is clear that where the rate-of-change of frequency of the tracking course is maximum at the high frequency end of the range, the blink rate is at its minimum. But the corresponding point at the low frequency end is not so clear. This asymmetry may be due to the fact that the task was actually different at the two ends, the "beat notes" being clearer at the high than at the low end of the frequency range. The "beat notes" served as a zero error signal provided the tracking rate demanded of the subject was not too high, when he had not time to use them.

The mean blink rate for all the fifteen subjects during the audio-tracking was also determined, and was found to be 18·49 blinks per minute. The Rest rates for these subjects were not determined, but this Mean may be compared with the average Rest rate of the forty subjects of the previous experiments, which is 20·62 blinks per minute.

The blink rate during audio-tracking was thus almost the same as the rate during maze learning. These means are markedly below the rate during trials of the number-adding experiment, which is not significantly different from the Rest rate. The reason for this exception in the case of the number-adding is not clear; it might be suggested from the result of this audio-tracking experiment that the trial mean is related to the degree of continuity of the task.

Any attempt to use blink rate as an index should take into account the degree of continuity of the task. Quantitative data should be found for the parameters of the rate-of-change of blink rate with changing conditions. The Audio Tracking experiment might provide a technique for studying the characteristics of the blink mechanism in following changing task conditions.

Both advice and help on the statistics were freely given by Miss Violet Cane and by Mr. N. Webb. Mr. W. Cunningham built the carriage for the audio-tracking recorder, and helped with the suction electrodes. Dr. N. H. Mackworth gave advice and encouragement throughout.

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THE APPARENT LENGTH OF A LINE AS A FUNCTION OF ITS INCLINATION*

BY

WILLIAM T. POLLOCK and A. CHAPANIS

(*From the Psychological Laboratories, The Johns Hopkins University, Baltimore, Maryland*)

In the first of two experiments reported here, subjects adjusted the length of a variable line until it appeared to be as long as a standard line. There were two sizes of standard line, 3 and 6 inches, and each was shown vertically for some trials and horizontally for others. The variable line was presented in each of the 10° positions from 0° (horizontal), through 90° (vertical), to 170°. The principal results of the first experiment are:

(1) Vertical lines look longer than horizontal lines of the same length, but lines tilted 20°–30° to the left of vertical look longer than lines in any other orientation. The results are asymmetrical, because lines tilted to the right of vertical do not look as long as those tilted to the left of vertical.

(2) The variability of the settings increases as the angle increases between the standard and variable lines.

(3) When they are expressed in percentage terms, the data obtained with the 3- and 6-inch standards are virtually identical, i.e. the data for the 3-inch standard can be made to match those for the 6-inch standard simply by doubling the former.

(4) There are enormous differences among subjects in the patterns of settings made at the various angles. A few subjects apparently experienced no illusory effects since they adjusted the variable line to about the same physical length irrespective of its orientation. Other subjects showed exaggerated overestimations of the variable line for vertical and near-vertical positions.

In the first experiment, the variable line was always to the left of the standard, and it was natural to assume that this position effect had somehow produced the asymmetry noted in paragraph 1 above. This hypothesis was tested in the second experiment which alternatively showed the variable line above, below, to the right of, and to the left of the standard line. The results of this experiment generally confirm the data of the first experiment in showing that lines tilted 20°–30° to the left of vertical look longer than lines tilted to any other position. In addition, the second experiment shows that this asymmetry in the results is not a function of the relative positions of the variable and standard lines. In general, however, overestimations of length are smaller when the two lines are one above the other, greater when the two lines are side by side.

I

INTRODUCTION

That people overestimate vertical distances as compared with horizontal distances was first pointed out by Wundt (2; pp. 158–163) in 1859.† He required his subjects to adjust a pair of dividers until the separation between the tips appeared equal to a standard distance—a 20 mm separation between two dots drawn on a piece of paper. Adjustments were made with the dividers held vertically, horizontally, or obliquely, and with the piece of paper rotated at different times so that an imaginary line between the two dots appeared at various inclinations with the horizontal.

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† Although Wundt's monograph is dated 1862, parts of the Beiträge had appeared earlier in various issues of the *Zeitschrift für rationelle Medicin.*

Wundt reports that no matter what the orientation of the dots or the dividers, vertical and near-vertical distances look uniformly longer than horizontal ones. The maximum effect occurred when the two dots were vertical and the tips of the dividers horizontal, or vice versa. The illusory effect under the former conditions amounted to about 25 per cent. of the actual distance involved, i.e. when the dots were vertical and the tips of the dividers horizontal, the latter had to be set 25·5 mm apart in order to match the 20 mm separation between the dots. Finally, Wundt reports that the patterns of over- or underestimation are symmetrical about the horizontal and vertical axes. Wundt, however, does not mention the number of subjects or trials he used to obtain these data.

A recent experiment by Shipley, Nann, and Penfield[1] used solid lines instead of open distances between points. These investigators presented subjects with pairs of lines equal in length, but at different inclinations. The inclinations varied from 0° (horizontal) to 90° (vertical) in 15° steps. The subject's task was to judge which of the two lines looked longer. Their results confirmed generally Wundt's data with one interesting exception: the maximum overestimation of length occurred at approximately 60° from the horizontal, i.e. lines tilted at 60° were reported even longer than those at 90°. Since their inclinations were confined to one quadrant, Shipley *et al.* have no data bearing on the symmetry of this function, and, of course, their technique did not permit them to quantify the amount of the illusion.

Although the overestimation of the vertical is a very old illusion, we have not been able to find any other experiments which have studied it at angles other than the vertical and horizontal. The experiments reported here are an extension of the same sort of work that Wundt and Shipley *et al.* have reported. Our experiments, however, differ from the earlier work in one or more of the following respects: we have tested more subjects, have controlled and specified our conditions of observation more precisely, have tested more angular positions, have used solid lines of two different basic lengths, and have quantified the apparent lengths of the lines.

II

EXPERIMENT I

Apparatus, Procedure and Subjects

Experimental Situation. The apparatus permitted the subject to adjust the length of a variable line until it appeared to match the length of a standard line. The subject sat 15 ft. from a 6-ft. square, white screen. Two black lines were located near the centre of the screen with the centres of the lines fixed in a horizontal plane. The line on the subject's right was of constant length; the other line could be varied in length by means of a cable and pulley arrangement. The variable line assembly consisted of a housing containing a slide, and was so constructed that the break between the housing and the slide was not apparent from the subject's position.

A screen brightness of 1·25 equivalent foot-candles was provided by a lamp in a reflector clamped to the subject's chair. The lamp was placed there to eliminate shadows which would be visible to the subject. No other lights were on during experimental runs.

Procedure. The basic experiment consisted of two parts. The first made use of a 3-in. standard line, and the second used a 6-in. standard. In both cases the lines were $\frac{1}{4}$ in. wide. For tests with the 3-in. standard, the centres of the standard and variable lines were 9 in. apart; for the 6-in. tests, the separation was 18 in.

The standard line was always shown in either the horizontal or the vertical position. The variable line was placed in one of the 10° positions from 0° to 170°. Since each position of the standard occurred with each position of the variable, a total of 36 comparison conditions resulted. In addition, each subject was given two trials under each condition, making a total of 72 judgments per subject.

The 72 trials were presented in a randomized order, i.e. positions of the standard and variable lines were arranged in a random sequence. The experimenter set the variable line at its maximum length on the odd-numbered trials and at its minimum length on the even-numbered trials. The subject was given unlimited time to equate the lengths of the two lines. Binocular viewing with uncontrolled fixation, but with no head movement, was permitted.

Subjects. All subjects were screened to eliminate those with poor visual acuity or marked astigmatism. The 20 subjects used in the 3-inch trials consisted of 8 females and 12 males, ranged from 17 to 26 years in age, and were composed of high-school, undergraduate and graduate students. The 20 subjects in the 6-inch trials were more homogeneous. They were 1 female and 19 male undergraduate students at Hopkins, ranging from 19 to 23 years in age. The 40 subjects were all different.

Results and Discussion

Constant Errors. Table I gives the constant errors for the judgments with both the 3- and 6-inch standard lines. The curves in Figure I show the constant errors for the horizontal standard. Subjects were quite accurate in equating the lengths of the two lines when they were in the same position. Thus, when the two lines were at 0° (horizontal), the constant error of the settings was nearly zero. But as the angle between the two lines increased, the constant error increased negatively. Note, however, that the greatest overestimation of length occurred when the variable line was tilted at an angle of 60°. It is also worth pointing out that percentagewise the data with the two different lengths of lines are virtually identical. Thus, the data obtained with the 3-in. standard can usually be made to match those with the 6-in. standard simply by doubling the former. (See also Figure II).

TABLE I

CONSTANT AND VARIABLE ERRORS OF THE SETTINGS AT THE VARIOUS ANGULAR POSITIONS OF THE VARIABLE LINE

Angle of variable line (degrees)	*Horizontal standard*				*Vertical standard*			
	3 inch		*6 inch*		*3 inch*		*6 inch*	
	Constant error	*Standard deviation*	*Constant error*	*Standard deviation*	*Constant error*	*Standard deviation*	*Constant error*	*Standard deviation*
170	−0·02	0·11	0·03	0·37	0·34	0·19	0·54	0·43
160	−0·10	0·14	−0·16	0·27	0·28	0·21	0·48	0·40
150	−0·16	0·12	−0·28	0·29	0·28	0·19	0·39	0·37
140	−0·15	0·15	−0·39	0·30	0·19	0·14	0·20	0·32
130	−0·21	0·18	−0·44	0·30	0·13	0·15	0·16	0·30
120	−0·24	0·16	−0·47	0·36	0·06	0·12	0·06	0·24
110	−0·26	0·18	−0·54	0·33	0·04	0·09	−0·02	0·23
100	−0·30	0·17	−0·48	0·36	0·01	0·10	−0·03	0·18
90	−0·25	0·16	−0·52	0·33	0·01	0·11	−0·02	0·16
80	−0·29	0·17	−0·53	0·36	−0·04	0·12	−0·09	0·20
70	−0·30	0·18	−0·50	0·36	−0·03	0·12	−0·13	0·21
60	−0·31	0·17	−0·69	0·33	−0·01	0·11	−0·04	0·22
50	−0·24	0·18	−0·59	0·32	0·05	0·12	−0·02	0·25
40	−0·25	0·16	−0·50	0·23	0·10	0·12	0·10	0·29
30	−0·22	0·14	−0·42	0·30	0·14	0·18	0·27	0·31
20	−0·16	0·14	−0·27	0·28	0·20	0·19	0·51	0·36
10	−0·08	0·11	−0·06	0·22	0·28	0·18	0·49	0·44
0	−0·02	0·10	0·10	0·23	0·27	0·20	0·58	0·47

Entries are in inches and each is based on 40 judgments.

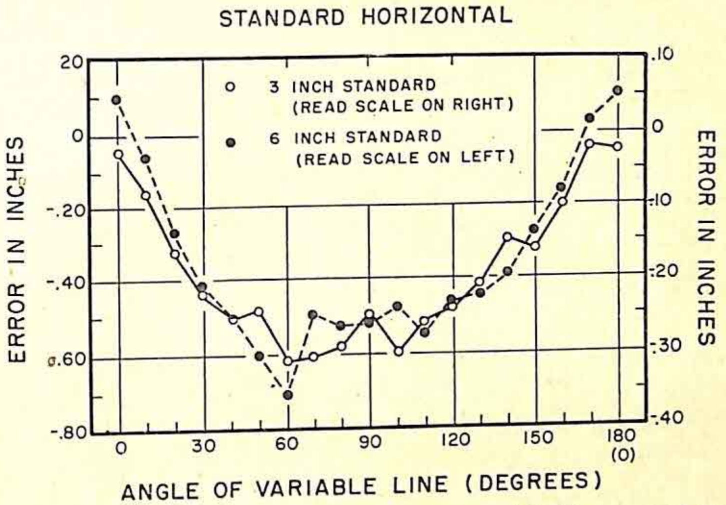

FIG. I.—Constant error curves for judgments made with the 3- and 6-in. standard horizontal lines. The negative error of 0·52 in. at 90° (solid point) means, for example, that a vertical line of 5·48 in. looked as long as the standard horizontal line which was 6 in. long. Each point is an average of 40 judgments.

Variability. As might be expected, the variability of the judgments was also dependent on the angular position of both the variable and standard lines. Generally the greater the angular difference between the positions of the standard and variable lines, the greater the variability of the judgments.

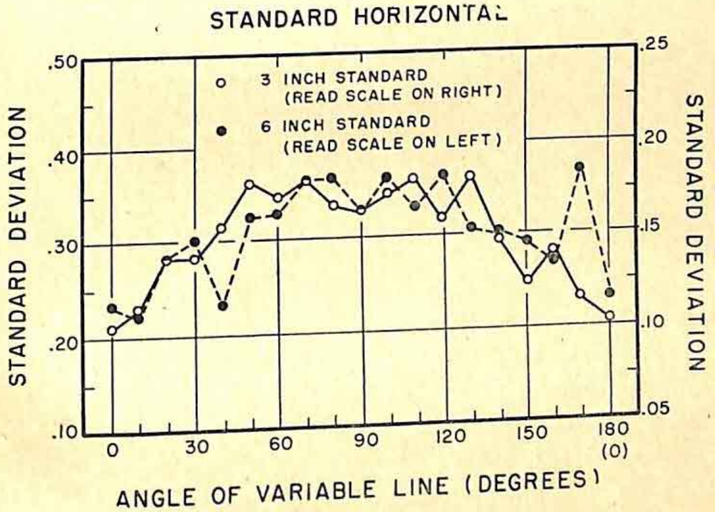

FIG. II.—Variable error curves for the 3- and 6-in. horizontal standard data.

Analyses of Variance. Because the variability of the judgments was dependent on the position of the standard line, separate analyses of variance were computed for the data obtained with the horizontal and vertical standards, and, for each of these in turn, separate analyses were computed for the data obtained with the 3- and 6-inch standards. The results are given in Table II.

In all cases the position of the variable line was a highly significant source of variance. Differences between the subjects are also highly significant. Finally, it is important to note the large percentage of the total variance contributed by the interaction between angular positions and subjects. That there were marked individual differences between subjects in the judgments they made is illustrated in Figure III. The curve at the top of the figure is for a subject who made approximately the same setting—and apparently experienced no illusory effects—for all angular positions of the variable line. At the bottom is the curve for a subject who responded by markedly overestimating the length of the variable line as the angular difference between it and the horizontal line was increased.

TABLE II

Analyses of Variance of the Constant Errors of the Settings in Experiment I
(See the data in Table I)

		Horizontal standard				*Vertical standard*			
		3 inch		*6 inch*		*3 inch*		*6 inch*	
Source of variance	*D.F.*	*Sums of squares*	*Estimated population variance*	*Sums of squares*	*Estimated population variance*	*Sums of squares*	*Estimated population variance*	*Sums of squares*	*Estimated population variance*
Between angular positions	17	6·09	0·36**	33·62	1·98***	10·50	0·62**	41·40	2·44**
Between subjects ..	19	10·03	0·53**	31·59	1·66***	6·38	0·34**	22·90	1·21**
Interaction	323	4·22	0·013*	19·91	0·062	6·58	0·020*	26·86	0·083*
Residual	360	2·87	0·0080	18·36	0·051	3·74	0·010	20·88	0·058
Total ..	719	23·20		103·48		27·19		112·04	

* Significant at the 1 per cent. level.
** Significant at the 0·1 per cent. level when tested against the interaction.
*** Significant at the 0·01 per cent. level when tested against the residual.

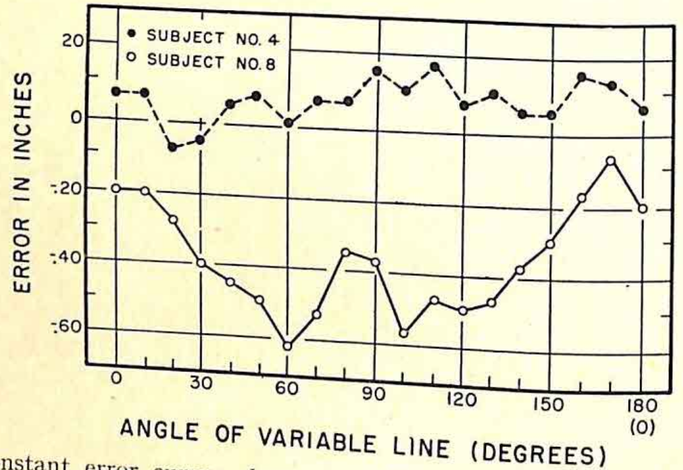

FIG. III.—Constant error curves chosen to illustrate the wide individual differences found in this experiment. Both subjects worked with a 3-in. horizontal standard. Each point is an average of two judgments.

Differences between Constant Errors at Various Angular Positions. The residual variance from the analyses in Table II can be used to test the significance of the difference between the constant errors for any two positions of the variable line. For example, the standard error of the difference between the constant errors for the 3-in. horizontal standard is given by:

$$\text{S.E.}_{\text{Diff.}} = \left(\frac{\cdot 0080}{40} + \frac{\cdot 0080}{40} \right)^{1/2} = 0{\cdot}020$$

Once this value is determined, the mean difference needed for a particular level of confidence may be computed from the t ratio. The values in Table III can be used to determine directly the significance of the difference between any two constant errors in Table I. For example, with the 3-in, horizontal standard, the difference between the constant errors at 90° and 60° amounts to 0·06 in. Table III shows that for this comparison a mean difference of 0·052 in. is associated with $p = 0{\cdot}01$. Thus, the difference of 0·06 in. between the 90° and 60° data is significant at $p < 0{\cdot}01$ for this condition. Similar comparisons may be made between other pairs of constant errors.

TABLE III

DIFFERENCES REQUIRED BETWEEN ANY TWO CONSTANT ERRORS IN TABLE I FOR SIGNIFICANCE AT THE 5 AND 1 PER CENT. LEVELS OF CONFIDENCE

	Horizontal standard		*Vertical standard*	
p	*3 inch*	*6 inch*	*3 inch*	*6 inch*
0·05	0·039	0·10	0·043	0·10
0·01	0·052	0·13	0·057	0·14

The columns of this table are to be used with the corresponding *Constant Error* columns of Table I.

Note in particular that, with a 6-in. horizontal standard, the difference between the 90° and 60° lines is significant at $p < 0{\cdot}01$; and that with the 3- and 6-in. vertical standards, the 90° and 70° data differ at $p = 0{\cdot}05$. All these comparisons involve angular positions of the variable line which produced larger negative constant errors than the comparable vertical line. Thus, we may conclude that lines inclined 20°–30° to the left of vertical look longer than any other lines of equal length.

A perplexing feature of these data is that lines inclined 20°–30° to the *right* of vertical produce less overestimation than lines tilted symmetrically to the left of vertical. A possible explanation of this discrepancy might be in terms of the relative positions of the variable and standard lines. In this experiment the variable line was always to the left of the standard. This fixed relative position might possibly be the cause of the asymmetry in our data. Experiment II was performed to test this hypothesis.

III

EXPERIMENT II

Apparatus and Procedure

Apparatus. The apparatus used in this experiment was similar to that of Experiment I. The main differences were in the shape of the screen and the control of the length of the variable line by the subject.

The screen in this experiment was a white circle, 5½ ft. in diameter, mounted on a stand so as to permit rotation of the screen about its centre. The variable and standard line assemblies were those used in the 6-in. standard trials in Experiment I. They were located near the centre of the screen with their centres 18 in. apart. By rotating the screen assembly about its centre, the two lines could be placed in any position relative to each other.

In this experiment, the subject did not have direct control over the length of the variable line. Instead, he signalled the experimenter who was behind the screen, whether to lengthen or shorten the variable line.

Procedure. The design of this experiment was a confounded factorial and so differed considerably from the factorial design of Experiment I. In this experiment the variable line was tested only at angles of 0°, 30°, 60°, 70°, 80°, 90°, 100°, 110°, 120° and 150°. In addition, the variable line was tested above, below, to the right of, and to the left of the standard line. The standard line was always horizontal.

The final variable tested here was the direction of movement of the slide in the variable line. In Experiment I, the slide of the variable line assembly always moved upward as it got longer. In this experiment both directions of slide movement were tested.

These three variables—ten angular positions of the variable line, four relative positions of the variable and standard lines, and two directions of movement of the slide—produce 80 experimental conditions. The conditions were assigned to four groups of ten subjects each; the subjects of any particular group worked with the 20 combinations of angular positions and directions of slide movement but at a single relative position of the variable and the standard lines. In short, the variance between groups of subjects is confounded with the variance due to the relative positions of the lines.

The 40 subjects in this experiment were all male undergraduate students at Hopkins, ranging from 19 to 23 years in age. None of these subjects had participated in the first experiment.

Results and Discussion

The curve of constant errors for all judgments without regard to the relative position of the two lines is shown in Figure IV. This error curve is similar in shape to the constant error curves with a horizontal standard obtained in Experiment I. Note especially that there is still a pronounced asymmetry in the data. Lines tilted at 60° and 70° look longer than those tilted at 110° or 120°. Since this curve combines data for all four relative positions of the standard and variable lines, we conclude that the asymmetry cannot be ascribed to this factor.

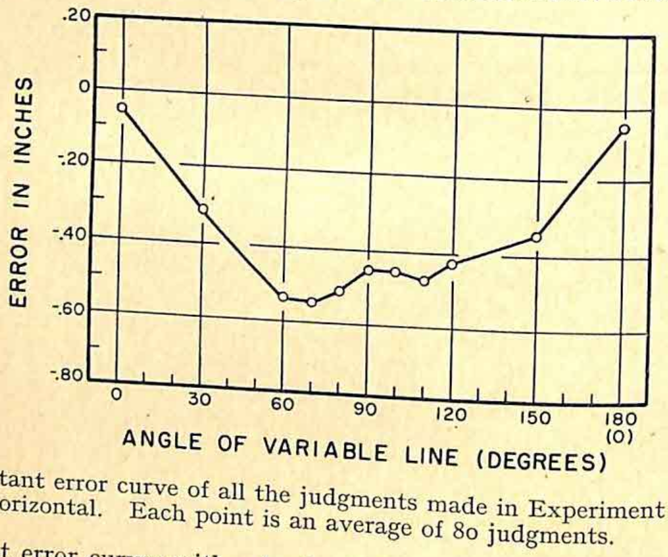

FIG. IV.—Constant error curve of all the judgments made in Experiment II. The 6-in. standard was horizontal. Each point is an average of 80 judgments.

Constant errors of the judgments as a function of the relative positions of the two lines are given in Table IV. Although they are less smooth, the data approximate roughly the data found in Experiment I. With one exception (standard to the right, which, incidentally, is the situation tested in Experiment I) the data show pronounced minima at 60°–70°, but not at 110°–120°. This also lends support to the conclusion that the asymmetry of the error curves of Experiment I was not a result of the fixed relative position of the variable and standard lines.

TABLE IV

Constant and Variable Errors of the Settings at the Various Angular Positions of the Variable Line

Angle of variable line (degrees)	*Position of standard relative to variable line*							
	Below		*Above*		*Left*		*Right*	
	Constant error	*Standard deviation*	*Constant error*	*Standard deviation*	*Constant error*	*Standard deviation*	*Constant error*	*Standard deviation*
150	−0·37	0·30	−0·28	0·26	−0·32	0·17	−0·41	0·29
120	−0·29	0·26	−0·26	0·41	−0·54	0·23	−0·62	0·35
110	−0·36	0·39	−0·30	0·30	−0·62	0·26	−0·64	0·32
100	−0·38	0·28	−0·21	0·38	−0·52	0·24	−0·72	0·38
90	−0·44	0·28	−0·20	0·59	−0·62	0·31	−0·57	0·25
80	−0·47	0·22	−0·26	0·38	−0·63	0·22	−0·71	0·28
70	−0·43	0·25	−0·34	0·34	−0·68	0·24	−0·73	0·29
60	−0·47	0·33	−0·40	0·32	−0·58	0·27	−0·70	0·30
30	−0·30	0·35	−0·24	0·27	−0·30	0·20	−0·41	0·32
0	−0·22	0·23	0·00	0·19	0·00	0·14	−0·01	0·09

Entries are in inches and each is based on 20 judgments.

The principal effect of changing the relative position of the lines is to shift the error function up or down. When the two lines are one above the other, overestimations are not as large as those found when the two lines are side by side (Fig. V).

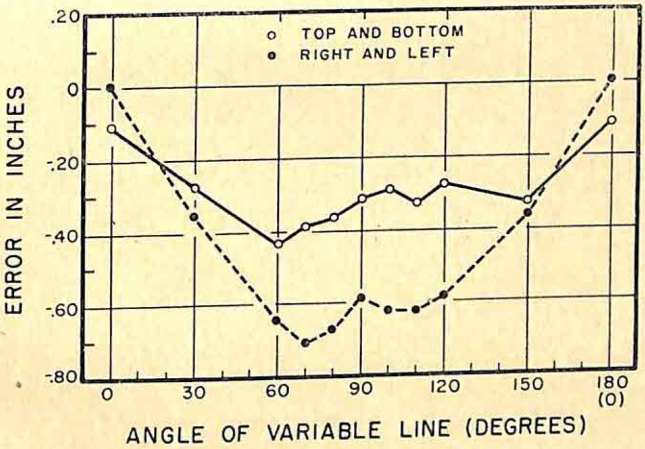

Fig. V.—The above and below data, and the left and right data, have been combined here. Errors of judgment are greater when the two lines are side by side; less when they are one above the other.

The variability of the judgments in this experiment was similar to that of Experiment I as may be seen from the standard deviations given in Table IV. Generally, the greater the angular difference between the two lines, the greater the variability of the judgments.

Table V presents the analysis of variance of the constant errors of the settings in this experiment. As expected from the previous experiment, the subjects contributed a significant source of variance to these data. Other significant sources were the relative positions of the lines, the angular positions of the variable, and the interaction of these two factors. The direction of movement of the slide in the variable line contributed an insignificant fraction to the total variance. The significant interaction of angular position and relative position indicates that the shapes of the error functions vary for different relative positions of the lines, but it does not negate the conclusion concerning the asymmetry of the curves with minima in the 60°–70° region.

TABLE V

Analysis of Variance of the Constant Errors of the Settings in Experiment II
(See the data in Table IV)

Source of variance	*Sums of squares*	*D.F.*	*Estimated population variance*
I. Between subjects	37·70	39	0·97**
A. Between relative positions of the two lines	10·48	3	3·49*
B. Between subjects within positions ..	27·23	36	0·76**
II. Between angle-direction of movement combinations	15·80	19	0·83**
A. Between angles of the variable lines	15·41	9	1·71**
B. Between directions of movement ..	0·02	1	0·02
C. Interaction: angles X directions ..	0·37	9	0·04
III. Interaction: subjects X angle-direction ..	48·34	741	0·07
A. Interaction: angles X positions ..	5·01	27	0·19**
B. Interaction: positions X directions ..	0·16	3	0·05
C. Interaction: angles X positions X directions	1·15	27	0·04
D. Residual	42·02	684	0·06
Total	101·84	799	

* Significant at the 1 per cent. level.
** Significant at the 0·1 per cent. level.

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MISCELLANEA

AN APPARATUS FOR MEASURING THE STEADINESS OF AIM OF A RIFLE

BY

K. H. SPRING

(*Ministry of Supply, Fighting Vehicles Design Establishment, Chertsey*)

THE apparatus described in this note was designed to measure the accuracy and steadiness with which a rifle may be aimed by a marksman. The only attachment to the rifle is a small plane mirror, which, with its supporting bracket, weighs about 2 oz. The apparatus is at present being used to study the physiological effect of various drugs on the aiming operation.

Optical System

A dual optical system is employed for measuring angular errors in elevation and azimuth (line), as shown in the schematic diagram. Considering the elevation system first, the condenser L_1 images the horizontal filament F_1 on to the slit S_1, which is in the focal plane of the lens L_3. This lens is a photographic objective of 36 in. focal length, and 6 in. aperture. The light from the slit therefore emerges from the lens as a collimated beam, a small portion of which is reflected by the plane mirror M_1 attached to the rifle R. This light re-enters L_3, and after reflexion at M_2 is imaged at A_1. After a further reflexion at M_3 the optical system L_4 and L_7 forms a second image of the slit S_1 at A_3. Rotation of the mirror M_1 about the axis *xx* causes this image to move up and down, thus recording a trace on the film moving behind the vertical slit S_3 in the camera C.

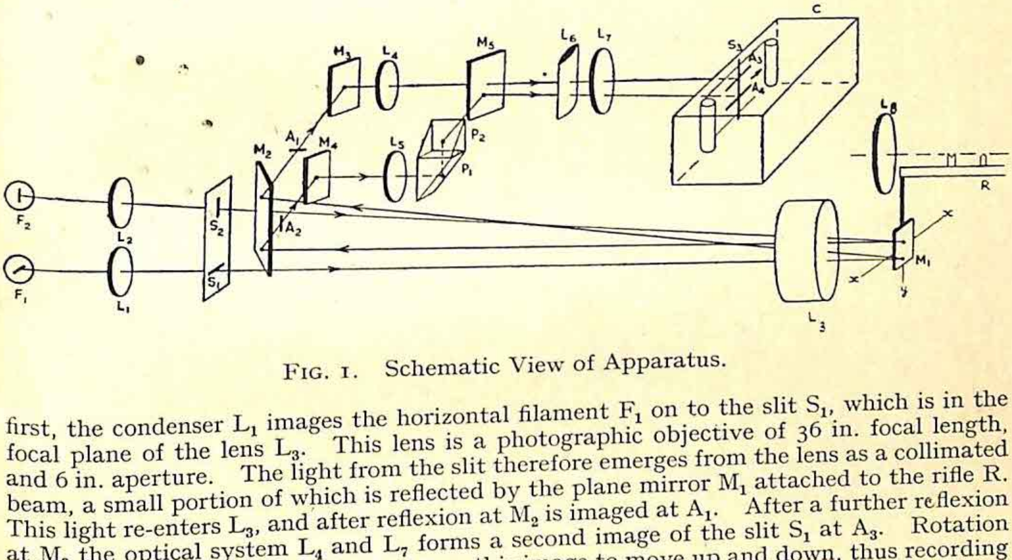

FIG. 1. Schematic View of Apparatus.

A similar optical system is used for errors in azimuth. An image of the filament F_2 is formed at A_2 and a further image, rotated through 90° by the prism system P_1 P_2, at A_4. Both beams are combined by the semi-reflector M_5, and the cylindrical lens L_6 draws out the slit images to a suitable length.

Operation

A telescopic sight is mounted on the lid of the apparatus to enable it to be aligned on to the aiming target, which in a laboratory or climatic chamber is normally 20 ft. away. Near the sight is a 20 ft. focal length collimator L_8, and the rifle is aimed by sighting through it. The target thus appears at infinity and the marksman has complete lateral freedom, within a 4-in. circle, without the introduction of angular errors. The diameter and position of the collimator are so chosen in relation to the lens L_3 that sufficient light is always reflected back by M_1 into L_3 to ensure an adequate photographic trace. Since the light emerging from L_3 is collimated, the marksman also has considerable longitudinal freedom.

Live shooting may be engaged in by removing the collimator L_8, leaving a 5-in. ring by which the rifle may be located. At the normal shooting range the permissible transverse movements cause very small angular errors. An electrical contact on the rifle trigger allows the instant of firing to be recorded on the film, by the momentary operation of a lamp placed in front of the camera slit. A correct steady aim is taken at intervals during a shoot, by supporting the rifle on a tripod, and aiming carefully, while the camera is run for a few seconds.

The sensitivity of the apparatus is such that 1° tilt of the rifle corresponds to about 8 mm. (0·3 in.) on the film. Errors of 1 minute may thus be measured fairly readily, and the angular range is about $\pm 1{\cdot}5°$ in both line and elevation.

Acknowledgment

The final optical arrangement and alignment of the apparatus were carried out by the author's colleagues, H. S. Soulsby and N. P. Barton. This note is published by permission of the Chief Scientist, Ministry of Supply, and the figure is reproduced by permission of Her Majesty's Stationery Office.

BOOK REVIEWS

Experimenta Psychologica. By E. RUBIN. Copenhagen: Ejnar Munksgaard. 1949. Pp. 356. 34s.

There are a number of reasons for recommending this book, containing fifteen papers, eleven of which are in German.

These papers are a cross-section of Rubin's experimental work, with an introductory paper on broader issues. Rubin died in February of last year, 64 years old, having been Professor of Experimental Psychology in Copenhagen since 1922. Although he published profusely, he is known almost exclusively for his demonstration of the reversible figure-ground effects contained within his dissertation of 1915. *Experimenta Psychologica* provides an opportunity to study Rubin's work on other subjects, such as Taste, Time, Real Movement, and above all, Touch. Many of the problems tackled by Rubin would no doubt appear in quite a different guise to-day. The experiments which form the basis for his studies on the perception of haptic straightness would perhaps be introduced and given a *raison d'être*, by relating the difficulties experienced by gunners in adjusting springs without the aid of vision. Rubin himself would presumably have been the last to refute this present approach—he investigated real movement in preference to apparent movement, because the latter seemed to him quite artificial, whilst the former was "real-life"—but it makes a pleasant change to read about these problems in a "pure" setting. Particularly so since I suspect that some of the effects which he mentions, and which I could not find referred to in the textbooks, may be of very real importance to-day.

It is conventional to think of Rubin as a pillar of the Gestalt school, and rather more sophisticated to express surprise that his classic should have been accepted by the Gestalt psychologists since it originated in G. E. Müller's laboratory. It is therefore something of a revelation how little Rubin seems to have associated himself with Gestalt psychology in any formal way. The reason was not that he was specifically anti-Gestalt, but rather that he seems to have been anti any sort of formalized school of psychology. He explains this in the first paper of this collection, and there are several references to this point of view scattered throughout the volume. Of particular interest in this respect is a comment made by Rubin in his criticism of Fröhlich's paper on Emfindungszeit. Having first shown how Fröhlich's conclusions may be shown to be false, he sets about to evaluate the paper from a more general point of view. In the last paragraph he points out that the way in which a number of Fröhlich's students had followed him faithfully and without offering criticism is of considerable importance to the psychology of the formation of schools in science. It would therefore be out of place to try and attach any of the usual labels to Rubin. He seems to have had that rare gift of being able to maintain an open mind even after having made one very great contribution to an immensely difficult subject. In these papers he tries to show the importance of this enviable state of mind and shows himself to be one of the few of his contemporaries to praise the introspections of the unsophisticated subject.

It was his ability to describe his experiences untrammelled by the then prevailing notions on perception which brought him to the frontline with his study of reversible figures and his definition of the properties of figure and ground. Here, then, is another reason for welcoming this book. Some of the problems posed originally by Gestalt psychologists have lost much of their meaning in present-day contexts, and it is hard to understand what all the fuss was about. In part, this is due to the fact that much of the work of the Gestalt school has now become generally accepted; in part it is due to the recent developments in physiology and electronics: we no longer expect to find anything like a one-to-one relationship between "objective stimulation" and "subjective experience." In the pages of this book the reader will find himself back in a time when these problems were real and meaningful, and he may pause to consider the state of physiology at that time. His conclusions may be both worrying and comforting. Worrying, because it makes one realize how easily psychologists can be led astray by placing too much emphasis on incomplete physiological models; comforting because one has the pleasant feeling that there may have been a time where psychological observation may have been ahead of physiological description.

Then there is Rubin's style. Scientific German is never very easy, but in these papers there is frequent relief in light and humane touches, and there are few accounts as charming as Rubin's description of how he dissected a photographic shutter, by way of experimentation on insight. In nearly all the papers Rubin uses the first person, and this in itself is a welcome change from the present habit of scientific impersonality in most psychological writings. With this goes the impression that Rubin was still dealing with human beings, and not merely with intact central nervous systems, unfortunately endowed with random variations.

This book is not an easy book to read. Like all of its kind it has to be read in bits. But it is well worth while reading and having. J. A. L.

The Sensations: Their Functions, Processes and Mechanisms. By HENRI PIÉRON. (Translated by M. H. Pirenne and B. C. Abbott.) London: Frederick Muller. 1952. Pp. xxiii + 469. 42s. net.

The modern study of sensation can be conveniently divided into two epochs. First—the classical phase of the nineteenth century, when expanding anatomical knowledge, simple fertile theory, exploitation of the technical resources offered by physics and the systematic use of the judgments of the conscious subject as experimental data brought rapid, convincing advances. This was a great age, and the later of its participants had some excuse for thinking, like their physicist contemporaries, that the main lines of their subject had been established, and only minor points remained to be cleared up. How restricted was the framework within which these achievements were possible became evident only gradually, as the second epoch took shape. This owes its inception chiefly, perhaps, to two factors. The first of these, largely inspired by comparative studies, and by Sherrington's reclassification of sensory function, was a broadening of the concept of the sense-organ, and consideration of its role in the biological economy of the organism at large. No longer could those particular organs, such as the eye, which seemed to serve as the "windows of the soul," be taken as the most adequate or suggestive pattern for them all. The second factor was the introduction of another, electrical, indicator of receptor-function quite independent of the use of conscious judgments. The extent to which these and other innovations are fundamentally disruptive of older preoccupations and approaches in the study of the senses cannot yet, perhaps, be fully recognized. The revolution and its wider consequences for psychology generally may turn out to be not incommensurate with that touched off in physics by atomic study in the eighteen-nineties. For it implicates basic problems of the relation of conscious awareness to organic function.

If there is justice in these remarks, the attempt at this juncture to give a unified coherent account of sensory function, as opposed to a comprehensive manual of experimental data, demands notable courage in addition to pervasive erudition and didactic skill. It goes without saying that Professor Piéron commands all of these three, and he has brought them to bear in a weighty synthesis under four rubrics, namely, *mechanisms of excitation, bases of qualitative discrimination, principles underlying the spatial and temporal aspects of sensation* and *principles underlying the perception of intensity*. While, under each of these headings, the different sensory modes are kept relatively separate, the major emphasis is upon the abstraction of common aspects in each of these four respects. Here is a clearly-conceived—not to say Cartesian—framework, and Professor Piéron shows remarkable skill (and even occasional ruthlessness) in tucking loose ends into it. This is a tremendous feat, for an enormous amount of material, ranging from the latest novelties, both *trans-* and *cis*-atlantic, to established archaisms, has been packed into the work. If unification in the study of the senses is desirable, none but Piéron could have demonstrated so well and so completely the virtues and limitations of formalism as a means to this end.

How, then, can the study of this remarkable book leave so marked a sense of frustration? This is not a very easy question to answer, but part of the difficulty may perhaps be expressed by saying that the significance of conscious judgments as the data of sensory research can no longer be considered as unequivocal. Nor can the simpler categories, such as those of spatio-temporal structure, in the form in which they emerge from the analysis of perceptual experience, be accepted as the most appropriate and suggestive in the study of end-organ function. On the one hand, with the advent of electrophysiological method, the problems of the end-organs themselves have become purely physical problems. Their investigation as such demands freedom to choose whatever concepts or models growing insight suggests. On the other hand, where conscious judgments are used as data (as used they still must be) we are now less tempted to forget than we used to be that we are no longer studying the sense-organ alone, but always an integrated sensory system—even though we may have coaxed that system temporarily into a simple stable mode of functioning. As relating to the end-organ itself, therefore, the data are always to some degree contaminated by central and ancillary factors whose complexity we can still only feebly guess at. It is, indeed, remarkable that so much progress in understanding the end-organs was made upon the basis of treating the central systemic elements as passive, faithful indicators of end-organ function.

A single example may serve to reinforce these comments. Classical psycho-physics tended, partly on account of the leisurely tempo of its methods, to emphasize the *steady-state* functioning of end-organs. It was not supposed, of course, that their response to transients and to changing stimulation could be derived directly, as in certain ideal physical systems, from knowledge of their steady-state behaviour. Nevertheless, intrusions of time-dependent characteristics were treated as peculiar departures from normal function—almost as imperfections. The nimbler methods of electrophysiology, on the other hand, especially as applied in a wide comparative field, have revealed numerous instances where the *dynamic* mode of function is clearly the important one—often, indeed, because the organ does nothing at all in response to an unchanging stimulus. The adaptive significance in many of these instances is apparent. It would seem, in fact, as if any fresh approach to a unified theory of sensation must be from a direction different from the classical one. It must not try to derive its generality from considerations which arise chiefly from the experience of the fortunate owners of those remarkable and quite special instruments, the human distance receptors.

The true value and great merit of Professor Piéron's book is thus to present, clearly and fully, existing knowledge of sensory functions within a framework so firm and sustained as to challenge its own basic interpretation, and so to stimulate fresh exploration on a fundamental level. No work at present could do more.

Those whose first acquaintance with the book dates from the French edition of 1945 will be glad to learn that it has been provided with those formalities so beloved of non-French readers—a bibliography and an index. Professor Piéron has also taken the opportunity of including new material which has accrued since that date. R. C. O.

Individual Differences in Colour Vision. By R. W. PICKFORD. London: Routledge and Kegan Paul. 1951. Pp. xviii + 386. 30s. net.

Experimental attacks, from a number of widely different directions, have wrought havoc among theories of colour vision during the past fifteen years. Almost every fresh fact revealed, whether by histologists, electro-physiologists or those working in the psycho-physical field, has demanded a fresh departure from simplicity of theoretical interpretation. It would be optimistic to suggest that the end of this phase is yet in sight. An essential concomitant of these developments is a renewed and closer study of the varieties of colour defect, and it is to this task that Dr. Pickford has devoted himself during the past eight years. His report of methods and findings is contained in this book. An immense amount of work has gone to the making of it, for not only has Dr. Pickford investigated the colour vision of a large number of normals, and been rewarded by a good collection of defectives, but he has pursued the latter's family connections and obtained much useful genetic data. Not content with this he has made a close study of the reaction of colour-defectives to the discovery of their disabilities. Here he reveals a fascinating field of minor psychopathology.

It may be said at once that the chief merit of the work is that it suggests, and supports the suggestion by careful experiment, that the real variety of partial colour defects cannot be encompassed by prevailing schemes of classification. If Dr. Pickford's findings are confirmed by other workers, the comfortably logical division of partial defectives into protanomalous, deuteranomalous and tritanomalous cannot survive in its present simple and widely accepted form. The chief shortcoming of the investigation is one which can hardly be entirely avoided in work of this kind. To obtain appropriate data in sufficient quantities the author was forced to work under "field" conditions. This demands simple and portable apparatus, a limitation which makes it difficult to compare on any absolute basis the results obtained with those of other investigators. Especially in the field of colour vision this is unfortunate, for conclusions may turn upon small points of technique; and, on the other hand, the defective subjects cannot easily be hunted down a second time and re-tested by other methods. Consequently the more absolute the terms in which the conditions of stimulation are defined, the more valuable the data on each defective are to later workers, and the easier it is to confirm or disprove any particular conclusions. Dr. Pickford used an anomaloscope of his own design—one which might well be widely adopted as a standard portable instrument. He added greatly to the scope and usefulness of this instrument by providing not only for the ordinary Rayleigh green–red equation with yellow, but also for the blue–yellow equation with green. Adequate constructional details of the apparatus are provided, but he is rather less explicit about the various conditions in which it had to be used. It is known, for instance, that unless a constant voltage supply be provided for the incandescent lamp of an anomaloscope, serious errors can be introduced by quite small fluctuations of mains voltage. It may be that the use of filters in place of the more conventional prism would diminish the amount of this error, but one would be glad of the author's assurance (which I cannot find in the book) that its possibility was not overlooked. The same applies to the question of ageing in the lamps used.

Dr. Pickford's findings on the classification of colour-defectives are of very great interest, and may be briefly expressed as follows. Supposing we have an individual who, in a general way, is classified as "red–green weak." One of three results may be found on studying the matches he makes between yellow and a mixture of green and red. He may, in the first place, show a very wide range in settings of the mixture which appear to him to match the yellow. But the *mid-point* of this range may be found to lie in the same, or nearly the same, place as the setting made by a normal individual. Secondly, the *range* of his matches may be nearly, or quite, as narrow as that of the normal subject, but its mid-point may be displaced well to one side of the normal. Thirdly, the range may be wide, and its mid-point displaced. On these findings, Dr. Pickford argues that a distinction must be maintained between the colour-*weak* and the colour-*deviant*. This conclusion is of considerable importance, and would not have been possible but for Dr. Pickford's patience in studying not only the range of matches made, but also their mid-points, and for his fruitful interest in individual cases. Its confirmation by other workers will be awaited with interest.

These considerations relating to classification, however, occupy only a relatively small part of the book. Another interesting line developed is that of the possibility of applying the methods of factor analysis to the interpretation of colour-test data, and especially to the problem of how many primary colours, or distinct colour-mechanisms, must be supposed to exist. Dr. Pickford's conclusion is that a minimum of four are required, but he is careful to emphasize that the value of the factorial method in this context has still to be finally assessed. It might perhaps be asked, for instance, how far the resulting factors relate to colour vision mechanisms, and how far, on the other hand, they are the expression of special features of the apparatus and procedure used in the tests.

Dr. Pickford has quite a lot to say about the genetic aspects of colour defect, and this is supported by much material of great value he has collected in the form of pedigrees. He makes clear how much more work of this kind remains to be done before a full understanding of the hereditable basis of colour defect is reached. Finally, he has collected intriguing information about the tendencies in colour-defectives to conceal, minimize, or preserve their ignorance of, their defects. This would seem to provide an admirable field for the study of the mechanisms of symptom-repression in general, since, except in special connections, there is no overt manifest disability to complicate the picture, and the repressive factors gain full scope.

Enough has perhaps been said to indicate that not only the specialist in colour vision will be attracted by this book. Dr. Pickford's warm interest in the more general psychological aspects of colour-defect will make it appeal to a great many outside this rather constricted field. And it should form the starting-point for a lot of fresh work.

R. C. O.

INDEX OF AUTHORS

Page

AMMONS, R. B.
Relationship of Motivation and Method of Testing to Distribution of Practice Phenomena in Rotary Pursuit 155

BURKE, L.
On the Tunnel Effect 121

CARPENTER, A.
Note on Variable Speed Mechanical Drive Systems 46

CHAPANIS, A., with Pollock, W. T.
The Apparent Length of a Line as a Function of its Inclination 170

CLEGHORN, T. E., with Darcus, H. D.
The Sensibility to Passive Movement of the Human Elbow Joint 66

CROOKES, T. G., with Hall, K. R. L., and Earle, A. E.
A Pendulum Phenomenon in the Visual Perception of Apparent Movement 109

DARCUS, H. D., with Cleghorn, T. E.
The Sensibility to Passive Movement of the Human Elbow Joint 66

DINSMOOR, J. A.
A Discrimination based on Punishment 27

EARLE, A. E., with Hall, K. R. L., and Crookes, T. G.
A Pendulum Phenomenon in the Visual Perception of Apparent Movement 109

GREGORY, R. L., with Poulton, E. C.
Blinking during Visual Tracking 57

GREGORY, R. L.
Variations in Blink Rate during Non-Visual Tasks 165

HALL, K. R. L., with Earle, A. E., and Crookes, T. G.
A Pendulum Phenomenon in the Visual Perception of Apparent Movement 109

HICK, W. E.
On the Rate of Gain of Information 11

HUNTER, I. M. L.
Discrimination Learning and Transposition Behaviour of Rats in a Water Tank Apparatus 91

JACKSON, C. V., with Zangwill, O. L.
Experimental Finger Dyspraxia 1

KNÖPFELMACHER, F.
Some Effects of Reward on the Strength of Position Stereotypes in the White Rat 78

POLLOCK, W. T., with Chapanis, A.
The Apparent Length of a Line as a Function of its Inclination 170

POULTON, E. C., with Gregory, R. L.
Blinking during Visual Tracking 57

REID, R. L.
A Test of Sensory Pre-Conditioning in Pigeons 49

Page

SCOTT BLAIR, G. W., with Sheppard, D.
Subjective Assessments of Firmness - - - - - - - - 101

SHEPPARD, D., with Scott Blair, G. W.
Subjective Assessments of Firmness - - - - - - - - 101

SPRING, K. H.
An Apparatus for Measuring the Steadiness of Aim of a Rifle - - - 179

YELA, M.
Phenomenal Causation at a Distance - - - - - - - 139

ZANGWILL, O. L., with Jackson, C. V.
Experimental Finger Dyspraxia - - - - - - - - 1